Noise Book

Makers of

ASTRONOMY

By the Same Author

MODERN COSMOLOGIES, A Historical Sketch of Researches and Theories concerning the Structure of the Universe

MODERN ASTRONOMY. its Rise and Progress

THE ROMANCE OF MODERN ASTRONOMY.

THE CHURCH AND SCIENCE.

&c , &c

Makers of
ASTRONOMY

BY

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OXFORD
AT THE CLARENDON PRESS
1933

OXFORD UNIVERSITY PRESS
AMEN HOUSE, E.C. 4
LONDON EDINBURGH GLASGOW
LEIPZIG NEW YORK TORONTO
MELBOURNE CAPETOWN BOMBAY
CALCUTTA MADRAS SHANGHAI
HUMPHREY MILFORD
PUBLISHER TO THE
UNIVERSITY

PRINTED IN GREAT BRITAIN AT THE UNIVERSITY PRESS, OXFORD
BY JOHN JOHNSON, PRINTER TO THE UNIVERSITY

PREFACE

THIS book is based on two courses of 'Elder Lectures' delivered at the Royal Technical College, Glasgow, in the winters of 1930–1 and 1931–2. It is published in the hope that it may serve not only as a popular introduction to the life-work of the chief modern astronomers, but also as a book of reference containing information not readily accessible to the general reader.

The book does not claim to be in any sense exhaustive. Reasons of space have necessitated the exclusion of many distinguished names. This exclusion has not been carried out, however, in any arbitrary way. Despite their contributions to astronomy, such men as Clerk-Maxwell, Doppler, and Helmholtz have been left out because they were first and foremost physicists; and among contemporary scientists Einstein and Jeans are omitted because they are primarily mathematicians. Other intellectual giants, such as Clairaut, Gauss, and Newcomb, have been passed over for the reason that their researches do not lend themselves to popular exposition.

It has been found possible to include only a few living astronomers, and these have been chosen because their work has resulted in some striking advance in astronomy. As the book is intended to be of a more or less popular character, I have dispensed with footnotes referring to the many authorities—books, periodicals, and papers—which have been consulted in its preparation.

H. M.

EDINBURGH
February 1933

CONTENTS

LIST OF ILLUSTRATIONS

I

THE PATHFINDERS

NICOLAUS COPERNICUS—TYCHO BRAHE—JOHANN KEPLER
GALILEO DE GALILEI—CHRISTIAN HUYGHENS

THE first of the long list of great men who are entitled to the designation of makers of astronomy was born at Thorn, on the Vistula, on 19 February 1473. Niklas Koppernigk, known to posterity by his latinized name of **Nicolaus Copernicus,** was the son of a well-to-do merchant of Thorn who had migrated from Cracow some twenty years previously. Thorn, afterwards incorporated in Poland, was then, along with Ermland and West Prussia, simply under the suzerainty of the Polish king, and was for long regarded as a Prussian town. Indeed, German writers have claimed that the founder of modern astronomy was a German. As to this there is no certainty, though it is not improbable that Hoffding was right in his contention that Copernicus 'sprang from a German family which had for many years been settled in Poland', for 'as far as we know he neither wrote nor spoke Polish'.

At the age of eighteen, in 1491, Copernicus was enrolled as a student at the University of Cracow, a seat of learning at which mathematics and astronomy were specially cultivated; and even at this time the stars had claimed his attention. After a three years' course at the University, Copernicus returned home to Thorn. Fate determined that his career was to be an ecclesiastical one. His maternal uncle, the Bishop of Ermland, decided to appoint him to a canonry in the Cathedral of Frauenburg; but at the time of his return from Cracow the canonry was not yet vacant, and the Bishop advised his nephew to fill in the intervening time by studying in Italy. In 1496, therefore, he set out for Italy, and was enrolled at the University of Bologna in January 1497. His studies here extended over three and a half years and embraced Greek

and philosophy. But of greater importance was the fact that he met with Domineco da Novara, a practical astronomer, from whom he learned the art of observing. Indeed at Bologna Copernicus made his first recorded observation—that of an occultation of Aldebaran, on 9 March 1497. On completing his studies at Bologna, Copernicus proceeded to Rome in 1500. Little is known of his stay there, but his disciple Rheticus recorded that he gave a course of lectures on mathematics. In 1501 he returned home to assume his duties as Canon of Frauenburg, in Ermland, and he took his seat in the Cathedral Chapter on 27 July. Almost immediately, however, he obtained leave of absence to go abroad again, in order to continue his mathematical studies and to take up medicine as well. In the summer of 1501 he arrived at Padua, where he studied both law and medicine, and in 1503 he received a doctorate of Canon Law from the University of Ferrara. In the spring of 1506, at the age of thirty-three, Copernicus finally left Italy and set his face homewards; and from 1506 until his death he made Frauenburg his home. His duties as canon were not arduous, and he had a great deal of spare time for the practice of medicine as well as the pursuit of astronomy. While conscientious in the discharge of his ecclesiastical duties, Copernicus' main interest throughout the next thirty-seven years was astronomy.

From this we are not to infer that he spent all his evenings out in the open watching the stars. Copernicus was not what we would call an observer. From time to time he made a few observations, with as great accuracy as the instruments of the time would allow, mainly of eclipses and planetary oppositions. His main work, however, was theoretical, and his chief object was nothing less than the formulation of a new system of the world less complex than that of Ptolemy, which was then universally accepted. The idea occurred to him early in life that Ptolemy's fundamental postulate was wrong, and that the Earth, instead of being the pivot of the entire

Universe, as Ptolemy thought, was merely one planet among others revolving round the Sun. Whether he was led to this view by reading of the 'heresies' of some of the more daring Pythagoreans, or whether the idea occurred to him independently, we do not know. He learned from Cicero, as he himself acknowledged, that Hicetas had believed the Earth to be in motion and that Philolaus and Heracleides had held views somewhat similar.

'Occasioned by this,' he said, 'I also began to think of a motion of the Earth, and although the idea seemed absurd, still, as others before me had been permitted to assume certain circles in order to explain the motions of the stars, I believed it would readily be permitted me to try whether, on the assumption of some motion of the Earth, better explanations of the revolutions of the heavenly bodies might not be found.'

During the long period of thirty years Copernicus was engaged in the working out of this daring hypothesis. In intellectual and academic circles his revolutionary views were quite well known, and at the request of one of his friends he drew up a short manuscript sketch of the heliocentric system for private circulation. In 1533 a verbal account of the new system was given to the Pope—Clement VII—by a certain scholar called Widmanstal. Three years later the Archbishop of Capua, a liberal Catholic churchman, who was a close friend both of Clement VII and of his successor Paul III, wrote to Copernicus advising him to make his new theory known to the world. By this time Copernicus had completed his great book *De Revolutionibus Orbium Coelestium*, but he feared the reception which might be accorded to it and the possible consequences of that reception to himself. Doubtless he realized that the comparatively liberal-minded Clement VII was not typical of the Church as a whole. At all events, he was deaf to the Archbishop's earnest entreaties, and the book remained unpublished.

In 1539, however, Georg Joachim Rheticus, Professor at

Wittenberg, who had become familiar with the revolutionary views of Copernicus, went to Frauenburg to gain first-hand information about the new system from its author himself. Despite the difference in religious opinion between them—for Rheticus was a Protestant and a professor in the centre of the Protestant world and Copernicus remained a Romanist—the two became intimate friends, and the younger man at last persuaded the older to have the book printed. Accordingly the manuscript was sent on to Rheticus, now a professor at Nurnberg, in 1542, and was published there in the following year. By a strange irony, an advance copy reached Copernicus on the very day of his death. Just after his seventieth birthday he was struck down with haemorrhage and paralysis, and he passed away on 24 May 1543.

De Revolutionibus takes rank among the world's epoch-making books. In the nature of the case it must of necessity have created an intellectual hurricane; and it was because of this that the man on the spot responsible for its publication sought to blunt the edge of the new doctrine. This was Andreas Osiander, the well-known Lutheran theologian, to whom Rheticus entrusted the publication of the book on his own appointment to a chair in the University of Leipzig. Osiander, in a preface which Giordano Bruno said could only have been written by one ignorant ass for the benefit of some other asses, explained that the doctrine contained in the book was purely hypothetical, as no one could expect astronomy to give certain knowledge. Probably this preface, which many people believed to have been written by Copernicus himself, did delay the breaking of the storm. Copernicus, however, was by this time beyond the reach of hostile critics, lay or clerical.

Many of the views set forth in the book were in agreement with those of Ptolemy. In his first chapter Copernicus concluded that the universe is spherical, 'partly because this form, being a complete whole needing no joints, is the most perfect

of all; partly because it constitutes the most spacious form, which is thus best suited to contain and retain all things; or also, because all discrete parts of the world, I mean the Sun, the Moon, and the planets,[1] appear as spheres.' In the second chapter he repeated the classical arguments for the sphericity of the Earth. In Chapter IV he showed that 'the motions of the heavenly bodies are uniform, circular, uninterrupted, or are made up of combined circular motions.' In the fifth chapter he cautiously considered the possibility of the Earth having a circular motion of its own, giving rise to the apparent diurnal motion. In his eighth chapter he refuted the objections to the hypothesis that the Earth is in motion. Having shown that 'nothing stands in the way of the movability of the Earth', Copernicus in his ninth chapter sought to investigate 'whether it also has several motions so that it can be considered one of the planets'.

"That it is not the centre of all the revolutions is proved by the irregular motions of the planets, and their varying distances from the Earth, which cannot be explained as concentric circles with the Earth at the centre. Therefore, since there are several centre points, no one will without cause be uncertain whether the centre of the Universe is the centre of gravity of the Earth or some other central point. I, at least, am of the opinion that gravity is nothing else than a natural force planted by the divine providence of the Master of the World into its parts, by means of which they, assuming a spherical shape, form a unity and a whole. And it is to be assumed that the impulse is also inherent in the Sun and the Moon and the other planets, and that by the operation of this force they remain in the spherical shape in which they appear; while they, nevertheless, complete their revolutions in diverse ways. If then the Earth, too, possesses other motions besides that around its centre, then they must be of such a character as to become apparent in many ways and in appropriate manners: and among such possible effects we recognize the yearly revolution. If one admits

[1] This was not then known for certain, for the telescope had not been invented.

the motionlessness of the Sun, and transfers the annual revolution from the Sun to the Earth, there would result, in the same manner as actually observed, the rising and setting of the constellations and the fixed stars, by means of which they become morning and evening stars; and it will thus become apparent that also the haltings and the backward and forward motion of the planets are not motions of these but of the Earth, which lends them the appearance of being actual planetary motions. Finally, one will be convinced that the Sun itself occupies the centre of the Universe. And all this is taught us by the law of sequence in which things follow one upon another and the harmony of the Universe; that is, if we only (so to speak) look at the matter with both eyes.'

Three years and seven months after the death of Copernicus, the second of the great pioneers of modern astronomy was born. This was Tycho Brahe, without whose labours the Copernican system would of necessity have remained as an unconfirmed hypothesis—although, by a strange paradox, Tycho Brahe remained the convinced opponent of the system of the world which his observations were to establish on a firm foundation.

By the time of Tycho's birth, a century had elapsed since the revival of astronomy in Europe. In the interval a considerable amount had been accomplished. The labours of the ancients had now become accessible in the originals; trigonometry had greatly facilitated astronomical computations. Copernicus had shaken the implicit faith of learned men in the complex Ptolemaic system, and had offered the world a simpler alternative system, while new tables of the planets had been computed. But this was practically the sum of a century's progress. Much remained to be done.

'No astronomer', says Dr. Dreyer, 'had yet made up his mind to take nothing for granted on the authority of the ancients, but to determine everything himself. Nobody had perceived that the answers to the many questions which were perplexing astronomers could only be given by the heavens, but that the answers

would be forthcoming only if the heavens were properly interrogated by means of improved instruments, capable of determining every astronomical quantity anew by systematic observations.'

It is the supreme merit of Tycho Brahe that he saw the necessity of making such observations, and that he successfully carried through an extensive programme.

Tycho Brahe was born on 14 December 1546 at Knudstrup, in the extreme south of Sweden, which at that time was still Danish territory geographically and ethnographically. Unlike Copernicus, who sprang from what we would call the bourgeois class, Tycho was one of an aristocratic family. The Danish nobility was untitled, but the noble families were very ancient, and the genealogical tree of the Brahes ran back for hundreds of years, and there were branches of the family in Sweden as well as in Denmark. Tycho's father was Otto Brahe, a Privy Councillor and successively lieutenant of various counties, and latterly Governor of Helsingborg Castle, where he died at the age of fifty-four in 1571. Otto Brahe had a brother Jörgen (George) whose marriage had not been blessed with children. About the time that Otto Brahe was married, Jörgen extracted a curious promise from him; he made him swear that if ever he had a son, he would hand him over to his childless brother to be brought up by him. When Tycho was born, Jorgen at once asked Otto to fulfil his promise and to hand over the infant. But by this time the paternal feeling, which was dormant when the promise was made, asserted itself, and Otto and his wife flatly refused to hand their little son over. Jörgen thereupon let the matter drop; but he merely bided his time, and when, about a year later, a second son was born to Otto, he made a raid on his brother's home and carried off Tycho by stealth. The parents seem to have bowed to the inevitable rather than enter into a sordid family wrangle, and doubtless they realized that they had broken a promise, even though it was a promise which should never have been made. At all

events, they took no steps to compel Jörgen Brahe to return the child, and so it was in his uncle's home at Tostrup that Tycho was reared. In that home he was treated with every kindness, and perhaps received more privileges than he would have done in his father's household, where, in due course, brothers and sisters made their appearance. From the age of seven he was under the care of a tutor, who taught him Latin so thoroughly that he was able in after years not only to converse in the language, but also to write it gracefully. Some of his Latin poems were not without real merit.

Tycho was a little over twelve years old when he was entered at the University of Copenhagen. Here he devoted himself chiefly to rhetoric and philosophy; for the intention of his uncle, in which his father concurred, was to educate him for the career of a statesman. Men proposed, however, but destiny disposed; and on 21 August 1560 an eclipse of the Sun made a deep impression on Tycho, and from that day onwards astronomy claimed his attention. The eclipse took place approximately at the predicted time—for even the rough tables available in those days allowed of fairly accurate predictions of eclipses—and he left it on record that it struck him as 'something divine that men could know the motions of the stars so accurately that they could long before foretell their places and relative positions'. He was led to procure the *Ephemerides* of Stadius, a kind of epitome of the Ptolemaic astronomy, but this did not satisfy him, and he procured before the end of the year a copy of the works of Ptolemy. For the next three years he occupied himself chiefly with astronomy and mathematics.

At the close of his period at Copenhagen University, his uncle, who had regarded his keen interest in astronomy with somewhat critical eye, decided to send him to a foreign university and selected Leipzig. Jorgen Brahe chose a young man of great ability, Anders Sorensen Vedel, to accompany Tycho as tutor. Vedel was only four years older than Tycho

and was young enough to be a friend and companion as well as a tutor. Vedel afterwards became Royal Historiographer of Denmark, and the friendship between him and the King's Astronomer was a lifelong one. Vedel was under instructions from Jörgen Brahe to see that Tycho kept to the study of law, for which purpose, indeed, he had been sent to Leipzig. Tycho was, however, determined to follow out astronomy, and although some temporary coolness developed between tutor and pupil, Vedel was wise enough to see that the lad's interest in astronomy was no mere passing fancy, but was born of a real thirst for knowledge. And so Tycho became perfectly free to devote his time at Leipzig chiefly to scientific pursuits.

While at Leipzig Tycho procured two sets of astronomical ephemerides—the *Alphonsine Tables*, drawn up by direction of Alphonso X of Castile on the basis of the Ptolemaic system, and the *Prutenic Tables*, dedicated to the Duke of Prussia—whence the name—drawn up by Reinhold, a disciple of Copernicus. Tycho's interest was attracted by a conjunction of Jupiter and Saturn, an occurrence which was supposed to be of some significance from an astrological point of view. Tycho's observations of this conjunction led him to ascertain that the *Alphonsine Tables* were a month in error, while those based on the new system were only a few days out. This does not appear to have prejudiced him in favour of the Copernican system, but it did convince him that 'only through a steadily pursued course of observations would it be possible to obtain a better insight into the motions of the planets and decide which system of the world was the true one'. And with very rudimentary instruments—compasses and cross-staffs—he began practical observation. Despite his pioneering attitude in this respect, he was in other ways the child of his time, and at this stage of his career appears to have believed in astrology and to have worked out horoscopes for his friends.

In May 1565 Tycho left Leipzig and returned to Denmark, and soon after his return he had the misfortune to lose his uncle, who, despite the difference of opinion between them, was his best friend. His father and other relatives looked upon his taste for astronomy with disapproval, and accordingly he made up his mind to go abroad for a second course of study. He proceeded to Wittenberg, where he stayed for a few months, after which he removed to the University of Rostock. It was while resident at Rostock that an incident took place which has become more widely known than many of Tycho's greatest discoveries. On 10 December 1566 there was a dance at a professor's house to celebrate a betrothal, and Tycho was among the guests. Even at this early stage Tycho had an ungovernable temper, which was, indeed, in later years to prove his undoing. There was another Danish nobleman, by name Manderup Parsbjerg, present at the dance, and the two young men quarrelled. According to Gassendi, who in the following century collected many facts about Tycho and his work, the dispute had a very unromantic origin. They argued as to which was the best mathematician.[1] The quarrel was renewed at a Christmas party, and the two challenged each other to a duel. As a result of this duel, fought with swords in perfect darkness, part of Tycho's nose was cut off; he had the lost piece replaced by a composition of silver and gold. Gassendi stated that he had been told on good authority that Tycho always carried with him a small box with ointment which he frequently rubbed on his nose, possibly to prevent the artificial piece from falling off.

At Rostock Tycho made a considerable number of observations. Despite the disapproval of his relatives, he was becoming well known as a student of science. And in 1568 King Frederick II, a truly enlightened monarch, made a formal promise to grant him the first vacant canonry at the Cathedral

[1] According to Dreyer, 'this is probably only gossip'. *Tycho Brahe*, p. 27.

of Roskilde, in Seeland. After the Reformation these canonries were not abolished, but secularized, and were used to provide sinecures for learned men. After a term of study at Basle, Tycho went to Augsburg, where he constructed for two wealthy merchants a quadrant for observation of the stars. In 1570 he left Augsburg and returned to Denmark on account of the serious and, as it happened, fatal illness of his father. On his father's death he fell heir to half of his property. Most of his time was now passed in the company of a maternal uncle at Heridsvad, near Helsingborg. This uncle was himself interested in science, more especially chemistry, and for two years Tycho was immersed in this study. His uncle had the oversight of the Abbey of Heridsvad, and permitted him to set up a laboratory in an outhouse of the Abbey. Here uncle and nephew worked together, most probably endeavouring to achieve the transmutation of baser metals into gold; for in those days chemistry and alchemy were synonymous terms.

Tycho's attention was recalled to astronomy by the unexpected appearance of one of the brightest temporary stars ever recorded. On the 11th of November 1572, while walking across to the house for supper, after a night's work in the laboratory, Tycho happened to glance upwards at the sky, and was amazed to notice a very brilliant star in the constellation Cassiopeia.

'Since I had almost from boyhood known all the stars of the heavens perfectly (there is no great difficulty in attaining that knowledge) it was quite evident to me that there had never before been any star in that place in the sky, even the smallest, to say nothing of a star so conspicuously bright as this. I was so astonished at this sight that I was not ashamed to doubt the trustworthiness of my own eyes. But when I observed that others, too, on having the place pointed out to them, could see that there was really a star there, I had no further doubts.'

Tycho was not actually the first to see this new star; it had

been noticed by others. But he was the only one to observe it systematically and to speculate concerning its nature. For the shining out of this strange and brilliant object was a scientific event of the first magnitude, and was a severe jolt to the defenders of the Aristotelian philosophy. For according to that philosophy, there could be no change in the realm of the fixed stars. In Tycho's own words,

'all philosophers agree . . . that in the ethereal region of the celestial world no change in the way either of generation or corruption takes place; but that the heavens and the celestial bodies in the heavens are without increase or diminution, and that they undergo no alteration either in number or in size or in light or in any other respect; that they always remain the same, like unto themselves in all respects, no years wearing them away.'

When first seen the star was as bright as Venus at maximum brilliance, and remained at this brightness for the greater part of November. In December it faded somewhat, but was still equal to Jupiter. During the next few months it steadily declined, and by March 1574 it became invisible to the unaided eye; and, of course, there were no telescopes in those days with which to follow it farther. Failing to find a measurable parallax, Tycho was driven to abandon the theory that the star was atmospheric in origin; it was manifestly far beyond the Earth, and so Tycho concluded that 'it is not some peculiar kind of comet or some other kind of fiery meteor become visible. For none of these are generated in the heavens themselves, but they are below the Moon, in the upper region of the air, as all philosophers testify.' At this stage Tycho believed in the sublunary nature of comets, but his belief was soon to receive a rude shock. The absence of parallax convinced Tycho, however, that the new star was not located in any of the lower spheres. And so he rightly concluded that 'this star is not some kind of comet or a fiery meteor, whether these be generated beneath the Moon or above the Moon, but that it is a star shining in the firmament

itself—one that has never previously been seen before our time in any age since the beginning of the world'. This conclusion he announced in his book *De Nova Stella*, published in 1573.

Soon after the publication of the book, Tycho offended his noble friends and relatives by marrying a peasant girl. Little is known of his wife, but the marriage would appear to have been quite a happy one. In 1575 he went abroad again, visiting Cassel—where the Landgrave Wilhelm was an enthusiastic student of astronomy Frankfurt-on-the-Main, and Basle, returning home via Augsburg and Ratisbon. On his return to Denmark a pleasant surprise awaited him. The King, Frederick II, who was, as already remarked, an unusually enlightened monarch, had heard accounts from the Landgrave of Cassel of his distinguished subject, and he offered him the little island of Hveen, in the Sound between Elsinore and Landskrona, in Scania, as a suitable residence where he could pursue undisturbed the study of astronomy. Tycho had little hesitation in accepting this munificent offer, and in the end of 1576 he entered into possession of Hveen. On this island he erected an observatory to which was given the name of Uraniborg—'the city of the heavens'. Here he laboured for over twenty years, and here by far the greater part of his work was accomplished.

Soon after Tycho's settlement at Uraniborg a bright comet made its appearance; and his observations of this comet were to be as epoch-making as his work on the new star. He discovered the comet on 13 November, and kept it under observation for two months. His instruments at this time were somewhat primitive, but they would have easily enabled him to measure the comet's parallax, had it been situated between the Earth and the Moon. The absence of an appreciable parallax proved conclusively that comets were not mere exhalations in the Earth's atmosphere.

Tycho's main work at Hveen, however, dealt with what is

called fundamental astronomy. With his finely constructed instruments he achieved the best possible results of pre-telescopic days. His observations of the Sun and Moon led to much more detailed knowledge of the apparent motions of the 'two great lights'. The planets, of course, came in for a vast amount of attention during the twenty years of his residence at Hveen; his planetary work was indeed the work on which his fame is chiefly founded. Tycho's early observations of planets, says Dreyer,

'were of course similar to those made by his predecessors. The ancients had generally fixed the position of a planet by mere alignment, or, if the distance from a star was small, by expressing it in lunar diameter, while conjunctions of planets *inter se*, or near approaches to fixed stars, were greatly valued as tests of theory. As long as Tycho only possessed few and small instruments, he naturally had recourse to these old methods, but he commenced also very early to adopt the method first used by Walther, of measuring the distance of a planet from two well-known fixed stars. At Hveen, he never quite gave up this method, but he chiefly depended on meridian altitudes and observations with the armillae, and even the difficult planet Mercury was observed on every occasion.'

Tycho was first and foremost a practical astronomer, and, as has been already stated, he realized that only the amassing of observations of planetary positions and motions could settle the vexed question of the true system of the world. This did not, however, prevent him from theorizing, and from setting forth an independent world-scheme. There can be no doubt that from his early years he was dissatisfied with the Ptolemaic system, and it would seem that but for certain theological presuppositions he might have accepted the new views of Copernicus, for which, indeed, he seems to have had a real sympathy. His planetary observations led him to reject the Ptolemaic theory outright. He believed himself, probably erroneously, to have determined the parallax of

Mars. In a letter to his friend Rothmann in 1589 he stated that he had found that Mars was nearer to the Earth than the Sun was, and that therefore the Ptolemaic system, which placed the orbit of Mars beyond the Sun, must be rejected. Dreyer points out that to measure the parallax of Mars was really beyond the power of his instruments, so that from an erroneous observation Tycho drew a correct conclusion.

But though he had now definitely abandoned the Ptolemaic system he was not prepared to accept the Copernican. He certainly perceived the futility of many of the objections which had been made to the Earth's annual and diurnal motions; thus the idea that there should be a violent commotion in the air if the Earth were rotating was rightly dismissed by him as absurd. At the same time, he felt that the balance of evidence was against the Copernican system. A stone falling from a high tower, he thought, ought to fall far from the foot of the tower, if the Earth really turned on its axis. Most formidable of all was the objection that if the Copernican system were true, each of the stars should show an annual parallax. True, Copernicus had been prepared to argue that the absence of parallax was due to the vast distance between the orbit of Saturn and the star-sphere. But Tycho was unwilling to allow for so great a distance as this. He placed Saturn at a distance of 50 million miles, and the star-sphere at a little under 60 million. Having rejected the Copernican system, the problem before Tycho was to find a system which, in deference to the supposed authority of Scripture, would conserve the immobility of the Earth and would yet possess all the advantages of the Copernican hypothesis. The 'half-way house' which he erected—the so-called Tychonic system—is now of historical interest only. It was promulgated in 1577, in his book on the comet of that year. According to the Tychonic system, the Earth is the centre of the Universe, and the centre also of the orbits of the Sun and Moon and the sphere of the fixed stars. The Sun,

however, is the centre of the orbits of the five planets, of which Mercury and Venus have orbits with smaller radii than the orbit of the Sun; while the orbits of Mars, Jupiter, and Saturn completely encircle the Earth. The system accounted for the irregularities in the planetary motions which Ptolemy had been compelled to explain by epicycles; while as to the outstanding irregularities which still required epicycles in the Copernican system, these could be similarly explained. Tycho would seem to have genuinely believed in his system, but he died too early to test the bearing of his own observations on the accuracy of the theory. In the opinion of one at least of his contemporaries, however, Tycho's labours established no system of astronomy. He had discredited the old system without establishing the new. So thought Mastlin, Kepler's old teacher, who, writing to his former pupil shortly before Tycho's death, remarked that Tycho had left hardly a shadow of astronomical science, and that only one thing was certain, namely that men knew nothing about astronomy. The Tychonic system was soon brushed aside by the advance of astronomy; the only purpose which it served was to provide a refuge for timid astronomers in France and Italy, who had broken with Ptolemy and were unable through fear of the Inquisition to follow Copernicus.

Tycho's residence at Uraniborg extended over twenty years. In 1588 he had the misfortune to lose his royal benefactor, King Frederick II, who died prematurely, leaving a son eleven years old as heir to the throne. After the king's death Tycho felt his tenure of Hveen to be somewhat precarious, and he certainly did not go out of his way to render it more secure by seeking to conciliate those who now held the reins of power. Despite a warm heart and an enlightened mind, the great astronomer had a violent temper; and this same temper which in student days had lost him part of his nose was now to lose him all his lands and also his pension. There was blame all round. After the death of Kaas, the

Chancellor, in 1594, the attitude of the Government to Tycho became less friendly, and unfortunately he had got into difficulties with some of his tenants on the island, who accused him of maltreating them. Accordingly, in 1596, one of his estates was taken from him, and in the following year his pension was stopped. A man of Tycho's high spirit could not be expected to submit to this, and within a few months he left Denmark for Germany. This still further angered the king, who took from him another source of income by depriving him of the canonry of Roskilde. Tycho wrote a letter to the king, pointing out that he could not possibly carry on his scientific work without means, and offered to return to Denmark. The letter was deferential in tone, though by no means slavish. But it did not have the desired effect. The king was hostile, and it became evident that Tycho must reconcile himself to exile from Denmark. He spent the winter of 1597-8 near Hamburg, and during this time he issued an account of his instruments and his work, with a short autobiography attached. This was circulated among influential personages, among them the Emperor and the Prince of Orange. The Emperor, Rudolf II, though an incompetent monarch, had a genuine interest in science, and he decided to avail himself of the services of the great astronomer. He invited him to become his Imperial Mathematician, with residence at Benatky, near Prague, and this invitation was accepted. The conditions of the appointment were that 2,000 florins be paid annually from the Imperial Treasury and 1,000 florins from the estates of Benatky.

To the castle of Benatky, twenty-two miles from Prague, Tycho now proceeded with his family; and to this place he brought his instruments. Once these were installed, he took up his scientific work where it had been broken off at Hveen. Looking around him for a young man who would act as his assistant, by a remarkable piece of good luck he came into touch with Johann Kepler, a young professor exiled from

Styria on account of his adherence to Protestantism. Thus was opened a partnership, very brief indeed, but of the greatest significance in the history of science. At the request of the Emperor, who expressed the desire to have his astronomer near him, Tycho Brahe left Benatky and settled in Prague. This was in 1600. But his life-work was drawing to a close. He was not happy in Bohemia; conditions there were unsettled, and religious strife seemed likely to break out with renewed activity. Evidently, too, his health was impaired, otherwise Kepler would hardly have said of a man in his early fifties that the feebleness of old age was approaching.

On 13 October 1601, while at supper in the house of a magnate of Prague, Tycho was taken suddenly ill. What the illness was it is not easy to determine; probably it was some acute inflammatory complaint. After five days' acute suffering he died on 24 October 1601, in his fifty-fifth year. On his death-bed he begged Kepler to finish the new tables of the planetary motions—to be called the *Rudolphine Tables*, in honour of the Emperor—as soon as possible. During his delirium he was frequently heard to repeat the words, 'ne frustra vixisse videar'—'O that I may not have lived in vain'. Most certainly he was justified in expressing such a hope; for his long series of observations, by far the most accurate ever made, passed on his death into the keeping of the one man in Europe who knew how to use them. It was this priceless legacy to which he fell heir that made possible the discovery by Kepler of the laws of planetary motion, and incidentally the final proof that the heliocentric theory, which Tycho had rejected, was the only possible system of the world.

Johann Kepler was born at Weil der Stadt, in Würtemberg, on 27 December 1571. It may be said without exaggeration that never did rare genius spring from an environment so unpromising. His father, Heinrich Kepler, was a drunken scamp. Son of a merchant of Weil der Stadt, who had served as burgomaster of that town, he had squandered practically

the whole of his share of the family fortune, and at the time of his famous son's birth was serving as a soldier in the Duke of Würtemberg's army. His wife, Catherine Guldenmann, was an evil-tempered, illiterate virago, who could neither read nor write, and who dabbled in witchcraft. When the future astronomer was five years of age, a friend for whom Heinrich Kepler was surety became bankrupt, and this bankruptcy wiped out the remnants of the family fortune. Heinrich was compelled to sell the little property which he owned; and on leaving the army soon after, he opened a tavern in the town of Elmendingen. Johann and his two younger brothers were removed from school to serve in this inn; and so the first job to which the future astronomer had to turn his hand was that of a pot-boy in a public-house!

Shiftless and dissolute though Heinrich Kepler was, he seems to have sensed the intellectual qualities of his son and to have had some ambitions for him. Accordingly, he decided to send him to a more advanced school, and on 26 November 1586 young Kepler, then nearly fifteen years of age, was admitted to a school at Maulbronn, maintained by the Duke of Würtemberg for the purpose of preparing boys for the University of Tubingen. Kepler passed through school and university with distinction and graduated Master of Arts before he was twenty. His university career was badly interrupted by successive illnesses—for his health from childhood was never robust—and by family vicissitudes. The tavern at Elmendingen proved a failure, and his father, unable to endure the company of his ill-tempered wife, deserted her and enlisted in the Austrian army, then fighting against the Turks. Meanwhile Frau Kepler, having got rid of her dissolute husband, quarrelled with all her own relations. It was well for Kepler that he was living away from home when this family storm was at its height.

Kepler's original intention in enrolling as a student of Tübingen was to prepare for the ministry of the Evangelical

Lutheran Church. But as time went on he found that the ecclesiastical atmosphere was uncongenial, and that the theologians with whom he came into contact were narrow-minded and dogmatic. So Kepler, himself of a deeply religious temperament, abandoned the idea of the ministry. Meanwhile another interest had crept into his life. The professor of mathematics at Tübingen was Michael Mästlin, one of the ablest astronomers of the day, who in a cautious and diplomatic way accepted the Copernican system. Certainly he taught the heliocentric theory to his students, and Kepler was one of the first of these to accept it. Kepler's interest in astronomy was now definitely kindled, and when in 1594 the lectureship in astronomy in the University of Gratz, in Styria, fell vacant, he applied for the post. To his surprise he received the appointment, and accepted it with, as he himself expressed it, 'many protestations that I was not abandoning my claim to be provided for in some other more brilliant profession'. At this stage he was interested in astronomy, but not enthusiastic. His chief interest was in philosophy. However, the die was cast, and he set to work to master his subject. And as he put it himself, 'diligent thought on these things was the occasion of still further thinking; until at last, in the year 1595, when I had some intermission of my lectures allowed me, I brooded with the whole energy of my mind on this subject'.

The first-fruits of this 'brooding' was a volume which bore the title *Prodromus Dissertationum Cosmographicarum continens Mysterium Cosmographicum*, published in 1596, before he was twenty-five. The publication of this book was largely due to his old Tubingen teacher Mästlin, who superintended its publication. In this book the youthful author nailed his colours to the mast and boldly declared for the Copernican system. In the first chapter he demonstrated quite explicitly that the observed facts of astronomy were all against the old hypothesis and in favour of the new; and he outlined his first

attempts to find some law binding together all the members of the Solar System. Even at this early stage he was convinced that, if such a law could be found, it would be possible for him to compute the elements of all the planets if the elements of one were known. In all this he was correct, but his attempt to solve the problem proved abortive, and his weird theory of the five regular solids is of historic interest only. Between the six planetary spheres there are five intervals, and adopting for the semi-diameters of the spheres the values given by Copernicus, Kepler found that the five solids fitted between the spheres in the following order—the cube between Saturn and Jupiter, the tetrahedron between Jupiter and Mars, the dodecahedron between Mars and the Earth, the icosahedron between the Earth and Venus, and the octahedron between Venus and Mercury. Kepler believed himself to be on the right track, and the observed facts seemed on the whole to confirm his theory. 'The intense pleasure', he wrote, 'I received from this discovery can never be told in words.' But it was no discovery at all, as the youthful author was yet to find out. The book, however, served one good purpose; it served to bring him to the notice of other astronomers. It introduced him to Galileo, and, what was more important, to Tycho Brahe. Both praised the ingenuity of the book. Galileo expressed his pleasure at meeting so powerful an associate in the pursuit of truth; by this time he had himself become a Copernican.

In 1597 Kepler married Barbara Muller von Muleckh, an heiress of somewhat moderate means, who at the age of twenty-three had had some experience of matrimony. For her first husband had died, and her second husband had divorced her. Her third marriage was evidently more successful than her second; but most probably this was due to Kepler's gentleness and patience. At all events she does not appear to have been an ideal wife, and it is plain that he had a great deal to put up with. Soon after the marriage,

misfortunes began to crowd upon him. He found that his wife's fortune was much less than her relatives had led him to expect, and that, indeed, he was less affluent as a married man than as a bachelor. His salary was small, and his circumstances very narrow. Then in 1598 he had the misfortune to lose his professorship. When he applied for and received the appointment, the University of Grätz was under Protestant control; but in the course of the civil and ecclesiastical strife then devastating Germany the town and university fell to the Roman Catholics, and Kepler and the other Protestant professors were expelled. He withdrew to Hungary until the storm blew past, and in 1599 he was invited by the Styrian Diet to resume his chair at Grätz; but he was reluctant to do this, for the general situation was very unstable.

Meanwhile there came about a great opportunity, and of this Kepler decided to avail himself. This was the chance of getting into touch with Tycho Brahe. After Tycho's settlement near Prague as Imperial Mathematician, he began to look around him for some promising young man who might assist him in his work, and he at once thought of Kepler, of whose misfortunes he had heard with regret. On 9 December 1599 he wrote him a long letter, expressing the hope that he might soon meet him, and while hoping that Kepler should not be driven to his service by misfortune but by his own free will and love of science, invited him to join him at Prague.

Kepler had meanwhile proceeded to Prague and had entered into negotiations with Tycho. There were some difficult problems of finance and status to settle before Kepler finally decided to become one of Tycho's assistants. He hoped to have a year or two's association with Tycho and then to return to his post at Grätz. But this was not to be. A Roman Catholic commission arrived at Grätz in August 1600, and every professor and lecturer got the option of becoming a Roman Catholic or leaving Austria within forty-five days. Kepler could not renounce his faith, and so he was

compelled to accept the post of assistant to Tycho on a permanent basis. He had stuck out for something more like a partnership, for above all he wanted access to Tycho's planetary observations. Instead of this he found himself one of three assistants. However, the senior assistant, Longomontanus, left Prague to take up an appointment in Denmark, and Kepler soon found that he had entered into something like the colleagueship he had hoped for—a colleagueship cut short within a year by the death of Tycho in October 1601. Tycho had a hot temper, and Kepler was proud and sensitive, but the partnership had been remarkably congenial. On his death-bed Tycho entreated Kepler to reduce his vast mass of observations and to finish the new planetary tables—the *Rudolphine Tables*—as soon as possible. And he added the vain hope that Kepler would demonstrate in this way the truth of the Tychonic system.

Kepler had now at the age of thirty fallen heir to the most precious series of scientific observations ever secured, and he was to devote the remainder of his life to extracting from this the true theory of the planetary motions. On Tycho's death he was appointed Imperial Mathematician, which post he retained until his death. But it was largely an empty honour, for his salary was small and paid irregularly. For years he was placed in the humiliating position, to quote his own words, of 'begging his bread from the Emperor'. Kepler was of necessity a mathematical astronomer first and foremost, but he did occasionally make observations. So exhaustive was his study of the temporary star of 1604 that it has been invariably referred to as 'Kepler's star', just as the star of 1572 has always been associated with Tycho Brahe. Comets, too, came in for a good deal of his attention. Kepler hailed the invention of the telescope with enthusiasm, and followed the pioneer work of his great contemporary Galileo with the greatest interest. And though he never constructed a telescope he devised a form of the refractor superior to that of

Galileo himself. But he was not by instinct an observer; and the few systematic observations which he did make were not allowed to distract him from his life's work—the reduction of the observations of Tycho and the effort to find the true laws of planetary motion.

After eight years' strenuous work, he published in 1609 his *Commentaries on the Motions of Mars*, in which he announced the first and second of the three 'laws' of planetary motion which bear his name. He demonstrated these laws in the case of Mars, and correctly surmised that they held good in the case of the other planets as well. The first law related to the shape of the orbit of Mars. Hitherto in all world-schemes, Ptolemaic, Copernican, and Tychonic alike, it had been assumed that these orbits, whether geocentric or heliocentric, must of necessity be circles, for the circle was the perfect curve. It was in order to conserve circular motion that the innumerable smaller circles—the epicycles—had to be postulated by Ptolemy, and were retained by Copernicus and Tycho; and Kepler himself, at first, strove to represent the planetary orbits on the orthodox pattern.

Having decomposed the apparent motions of Mars into two components, terrestrial and Martian, Kepler concentrated on the irregularities of the Martian orbit. His task was to get an orbit which would satisfy the observations of the planet's position taken by Tycho. Hypothesis after hypothesis was rigorously tested by observation, and at last he devised an elaborate geometrical scheme which represented the observations on the assumption that certain of Tycho's observations were in error to the extent of eight minutes of arc. A less careful calculator would probably have treated this error as negligible and would have put forward a system of the world even more complex than that of Copernicus; in which case he would have missed making one of the greatest discoveries ever made, and the progress of science would have been retarded. Strong though the temptation was to assume an

error on Tycho's part, Kepler decided that he could not make such an assumption.

'Since', he said, 'the Divine goodness has given to us in Tycho Brahe a most careful observer, from whose observations the error of eight degrees is shown in this calculation, it is right that we should with gratitude recognize and make use of this gift of God. . . . For if I could have treated eight minutes of longitude as negligible, I should have already corrected sufficiently the hypothesis . . . discovered in Chapter XVI.'

But these could not be neglected, and as Kepler pointed out after he had carried through the whole of this laborious investigation, 'these eight minutes alone have led the way towards the complete reformation of astronomy'. Deciding that the attempt to reconcile Tycho's observations with a circular orbit for Mars must be abandoned, he tried the hypothesis of some kind of oval orbit. At last he found to his great delight that the simplest of oval curves, the ellipse, completely satisfied the observations. There was now no appreciable discrepancy between observation and theory. Kepler was thus able to formulate his first law—'the planet describes an ellipse, the Sun being in one focus'. He now attacked the problem of the variation of the planet's rate of motion at different parts of its orbit. Mars, he found, moved more swiftly when near the Sun and more slowly when distant from it, and at length the truth dawned upon him that the area described or 'swept out' in any time by the line joining the Sun to Mars was always proportional to the time. And so he was able to formulate his second law. 'The straight line joining the planet to the Sun sweeps out equal areas in any two equal intervals of time.'

Familiarity with the outstanding achievements in astronomical history has perhaps blinded us to the supreme greatness of Kepler's work. Dreyer's eulogium is by no means an overstatement.

'The genius and astounding patience of Kepler had proved

that not only did this new theory satisfy the observations but that no other hypothesis could be made to agree with the observations, as every proposed alternative left outstanding errors such as it was impossible to ascribe to errors of observation. Kepler had therefore, unlike all his predecessors, not merely put forward a new hypothesis which might do as well as another to enable a computer to construct tables of the planets' motion. He had found the actual orbit in which the planet travels through space.'

The first and second laws had been definitely established only in the case of Mars, but he had little doubt that he had discovered the laws of planetary motion, and he assumed from this time onwards that all the planets moved round the Sun, and the Moon round the Earth, according to the same laws. It was not until 1618, however, that Kepler made the definite statement in his book entitled the *Epitome of the Copernican Astronomy* that the two laws were true for the other planets and for the Moon as well.

The first two laws, as we have seen, were announced in 1609. Ten years elapsed before the third was discovered. It was set forth in his book on the *Harmony of the World*. Ever since the publication of his *Mysterium Cosmographicum* he had been searching for some law binding together the various members of the Solar System. His strange mystical ideas about the five regular solids were the first-fruits of the earlier gropings of his mind on this question of the numerical relations of the planets. His mind continued to dwell on the subject, and after the enunciation of his first and second laws, he devoted his chief attention to it. For years he grappled with the problem, until at length on 15 May 1618, in a moment of inspiration it might almost be said, he discovered the famous third law, namely, that the squares of the periods of the revolutions of any two planets are proportional to the cubes of their mean distances from the Sun. And he was able to state that the law not only applied to all the planets but to the newly discovered satellites of Jupiter as well.

The discovery of this third law filled him with the greatest exultation, and he was not ashamed to express this and to 'let himself go' in words. Nowadays we do not find rhetoric of this sort in scientific treatises; yet it was manifestly the outpouring of his heart.

'What I prophesied two and twenty years ago, as soon as I had discovered the five solids among the heavenly bodies; what I firmly believed before I had seen the "Harmonics" of Ptolemy; what I promised my friends in the title of this book, which I named before I was sure of my discovery; what sixteen years ago I urged as a thing to be sought; that for which I joined Tycho Brahe; for which I settled in Prague; for which I have devoted the best part of my life to astronomical contemplations;—at length I have brought to light, and have recognized its truth beyond my most sanguine expectations. . . . It is now eighteen months since I got the first glimmer of light; three months since the dawn; a very few days since the unveiled Sun, most beauteous to behold, burst out upon me. Nothing holds me. I will indulge in my sacred fury. I will triumph over mankind by the honest confession that I have stolen the golden vases of the Egyptians, to rear up a tabernacle to my God far away from the confines of Egypt. If you forgive me, I rejoice; if you are angry, I can bear it. For the die is cast, the book is written, to be read now or by posterity; I care not. I can well wait a century for a reader, since God has waited six thousand years for a discoverer.'

The mechanism of the Solar System was now laid bare to men. The three laws enormously simplified the Universe. Epicycles and all other weird circles had been done away with; nature was shown to work on the simplest plan; the heliocentric system was placed on an unassailable foundation. It was proved beyond all manner of doubt by Kepler's discovery that the planes of all the planetary orbits pass through the centre of the Sun. This, Dreyer contended with justice, ought to be called Kepler's first law.

The Harmony of the World was published in 1618. In the course of the next three years Kepler produced two other

volumes of considerable value. His *Epitome of the Copernican Astronomy* appeared in three parts in 1618, 1620, and 1621 respectively. This book was promptly placed by the Inquisition on the Index of prohibited works; for by this time the Church had declared war on the Copernican system. In this book he treated of the whole field of astronomy. Eclipses of the Sun and Moon were discussed, and he gave a correct explanation of the reddish tint of the Moon in eclipse. He also mentioned that a ring of light had been seen round the eclipsed Sun in 1567, and surmised that it was due to a solar atmosphere, a hypothesis much nearer the truth than that current in the eighteenth century—namely, that it was due to a lunar atmosphere. In the *Epitome* Kepler made an attempt to get an approximate value for the distance of the Sun. His failure to find any appreciable parallax in the case of Mars led him to the correct conclusion that the Sun must be considerably more distant than had hitherto been supposed. Kepler's value was inadequate; he placed the Sun at a distance of about 13 million miles, less than a seventh of the true value. Like Copernicus, Kepler believed the Sun to be the centre of the Universe. He computed the distance of the star-sphere at 420,000 million miles; at one stage he came near to accept the bold idea of Giordano Bruno that the stars were akin to the Sun, but he was unable to emancipate himself from medievalism, and to the end he believed the stars to be really fixed to a solid sphere, centred in the Sun, and 'two German miles in thickness'.

In his *Treatise on Comets*, 1619, Kepler clarified the intellectual atmosphere concerning these bodies. Tycho Brahe in 1577 had shown clearly that comets were not atmospheric phenomena but were genuine celestial bodies travelling in interplanetary space. Kepler believed that they travelled in straight lines, but he made no attempt to bring this vague idea to the test of calculation. He noted that comets' tails always point away from the Sun, and he suggested—in

anticipation of modern thought on the subject—that a comet's tail is formed by the rays of the Sun which, penetrating the body of the comet, drive off portions of the cometary material, so that comets are not permanent, but transient bodies.

In his book on comets Kepler dealt with the astrological significance of these bodies; and that he did this does not indicate that he really believed in astrology, but rather that the popular taste demanded something of the sort. All through his life Kepler was forced by the sheer necessities of the case to dabble in astrology. In order to defray the expense of publishing his scientific works he confessed in 1616, 'I have been obliged to compose a vile, prophesying almanac which is scarcely more respectable than begging, unless from its saving the Emperor's credit, who abandons me entirely, and would suffer me to perish with hunger.' Indeed, since his appointment as Imperial Mathematician in 1601, Kepler had been very badly treated by the authorities. His salary, hopelessly inadequate, was paid very irregularly, and in addition he suffered every kind of misfortune. In the year 1610 his wife and son were both taken ill and died; and Kepler was plunged into extreme poverty. He attempted to obtain the Chair of Mathematics at Linz, in Austria; but the Emperor prevailed on him to remain at Prague and promised to arrange for the regular payment of his salary, which promise was never implemented. In 1611 the Emperor Rudolf was forced to abdicate, and he died in the following year. His brother Matthias, who succeeded him, had little interest in astronomy, and did not oppose Kepler's transfer to Linz, which took place in 1612, and even allowed him to retain the post of Imperial Mathematician, with very occasional payment of salary.

Kepler's first experience of matrimony had not been altogether a happy one, but this did not deter him from deciding to marry again. He had young children and was in dire need of some one to look after them, as well as to attend

to his own creature comforts. At all events, he decided that in the matter of a second marriage he was taking no risks. Accordingly, he drew up what might be called a 'long leet' of his feminine acquaintances. The leet consisted of eleven, and in an amusing letter to a friend, Kepler sat in judgement on them. One was too old, another too fat, another too proud, yet another so ugly that 'she would be stared at in the streets'. So Kepler narrowed down the list until he made a final selection, an orphan girl of humble origin but good education, whom he married in 1612. The marriage proved to be a very happy one. But despite the appointment to the Linz chair Kepler's circumstances remained straitened; and domestic troubles continued to harass him. His old mother was a constant source of embarrassment. In 1620 she was apprehended on charges of witchcraft and attempted poisoning, and had a very narrow escape from capital punishment. Kepler succeeded in moving the authorities to clemency. After her release his mother brought an action against her accusers, but the proceedings were stopped by her death in her seventy-ninth year. Owing to his sturdy Protestantism, the tenure of Kepler's chair at Linz was always an uncertain one. Nevertheless, he refused the offer of a chair at Bologna, and likewise the invitation of the English ambassador to settle in England as a pensionary of the king. In 1626, for the second time in his life, he was driven from a chair by religious persecution. He withdrew from Linz to Ulm, where he resided for three years. He still retained, however, the office of Imperial Mathematician, and remarkably enough received at this time certain instalments of his salary. This enabled him to complete and publish the tables based on Tycho's observations, which that great astronomer had charged him to carry to completion. These were the *Rudolphine Tables*, published at Ulm in 1627, which were for over a century the standard astronomical ephemerides.

In 1629 Kepler received an invitation from the Duke of Friedland to take up his abode at Sagan in Silesia. His new patron secured for him a chair in the University of Rostock. This Kepler accepted, and the future seemed bright. But his life's work was done. His constitution was enfeebled by illness and anxiety. His salary from the Imperial Treasury was badly in arrears, and he decided to go in person to Ratisbon in Bavaria, where the Imperial Diet was convened to meet, to make a personal appeal for payment. His pleadings for what was common justice fell, however, on deaf ears. His health was completely broken by the journey and its disappointing issue. He contracted a severe chill and died, probably of what would now be called pneumonia, at Ratisbon, on 15 November 1630, in his fifty-ninth year. He was buried in St. Peter's Churchyard in Ratisbon.

A man of superlative genius, something of a mystic, of a deeply religious temperament, Kepler will be remembered first and foremost as the brilliant calculator and patient investigator who, through long years of mental toil, unveiled the working of the Solar System to his fellow men. Arago, the French astronomer, did not exaggerate when he said, 'The glory of Kepler is written in the heavens: the advances of science can neither diminish nor darken it, and the planets by the ever-constant succession of their regular movements will proclaim it from age to age.'

Galileo de Galilei, the younger contemporary of Tycho Brahe and the older contemporary of Kepler, was born at Pisa on 15 February 1564. He came of an ancient family, of which no fewer than fourteen had filled high offices in the government of the Republic of Florence between 1343 and 1528. The original surname of the family was Bonajuti, but for some reason this was exchanged for the name Galilei in 1543. Vincenzio de Bonajuti de Galilei, the father of the great astronomer, belonged to a branch of the family which had suffered from misfortune, and was engaged in trade as

a cloth merchant. He appears to have been a man of rare intellectual gifts, a talented musician and a man of wide culture, a mathematician of considerable power, and a good classical scholar. In a passage in one of his books on music he used words which indicate that his son, the astronomer, had inherited the spirit of free inquiry. 'It appears to me,' wrote Vincenzio, 'that they who in proof of any assertion rely simply on the weight of authority without adducing any argument in support of it act very absurdly. I, on the contrary, wish to be allowed freely to question and freely to answer . . . as well becomes those who are sincerely in search of truth.' This was the claim which his illustrious son was to make in after years.

Galileo, who was the eldest of a family of three sons and four daughters, received his early education in Pisa, partly from his father, partly in a private school kept by a friend. At the age of twelve he was sent to the monastery school of Vallombrosa, in order to specialize in classics. When, however, Galileo began to show signs of an inclination towards a monastic life, his father, who had a different career in view for him, removed him from Vallombrosa. This was in 1579, when the future astronomer was fifteen years of age.

Vincenzio Galilei, despite his abilities, was in straitened circumstances all his days, and did not feel able to afford a university education for his son. His idea was for Galileo to follow a commercial career and to become a cloth-dealer. But Galileo had other ambitions, and even at this early stage he had been experimenting with mechanics and had constructed several toy machines; he also, as a lad, excelled in music, painting, and drawing. His father accordingly decided at whatever sacrifice to send him to the University of Pisa, where he was enrolled as a student of medicine when seventeen years of age. Right from the beginning of his university career he came up against the conservatism and traditionalism of the teachers. He was his father's son—a true 'chip of the old block'—taking nothing on authority.

Galileo had no particular desire for the profession of medicine. His interests were in mathematics and experimental science, but his father was not in favour of such a career for his son, owing to the beggarly remuneration attached to scientific posts. The Professor of Mathematics at Pisa, for instance, received a sum equivalent to £13 a year. Nevertheless, Galileo was not to be deterred from entering upon a scientific career, and his father was too wise to forbid him. For a time, after leaving the University, he eked out his living by giving private tuition in mathematics and mechanics. After applying unsuccessfully for professorships at Bologna, Rome, Padua, and Florence, he succeeded in obtaining, at the age of twenty-five, the Professorship of Mathematics in his old University of Pisa.

The appointment was for a term of three years, and was renewable, but the young professor was not even allowed to complete his term. For in the course of three years he succeeded in arousing against himself the forces of reaction, prejudice, and superstition. 'The powers that were' in Pisa regarded Aristotle as sacrosanct, and would not believe that in any particular the great Greek philosopher could have been mistaken; and during his tenure of the chair Galileo was to commit the unpardonable sin of proving beyond all manner of doubt that Aristotle had been wrong in one of his statements concerning falling bodies. Aristotle had laid it down as an axiom that if two different weights of the same material were allowed to fall from the same height, the heavier would reach the ground before the lighter, proportionately to the difference in weight. It is somewhat remarkable that Aristotle, who was a careful observer of nature, never thought it worth his while to try the experiment; but it is nothing short of extraordinary that during all the centuries which had elapsed since his time, no one else had ever thought of making so simple an experimental test. Galileo's own experiments, however, taught him that Aristotle had been wrong. Except

for a very small difference, due to the resistance of the air, he found two unequal weights fell in the same time. When he made this known, he was ridiculed: he had contradicted Aristotle. He was not content, however, to let the matter rest there. Accordingly, he announced that he would perform the experiment from the top of the 'Leaning Tower' of Pisa. One morning, before an assemblage of students and professors, priests, and philosophers, he ascended the tower, carrying with him a 10 lb. weight and a 1 lb. weight. From the top of the tower, he let them go. They fell together and struck the ground at practically the same moment. And yet, though with their own eyes they had seen the two weights strike the ground together, the assemblage persisted in maintaining that Aristotle was right and Galileo wrong. As a result of this and several other incidents, things were made so unpleasant for Galileo that he was forced to resign his post before his three years' tenure of the chair had expired. This was in 1592; in the year previous his father had died, leaving him as the head of the family, with his surviving brother and four young sisters dependent upon him. For some months Galileo had a stern struggle against poverty; this was the price he had to pay for his passion for truth.

In September 1592 he applied for the vacant chair of Mathematics at Padua, in Venetia, and had the good fortune to be appointed. This appointment opened the happiest period in Galileo's life. The Republic of Venice was quite the most enlightened of the Italian states. Its rulers were determined to allow freedom of thought and inquiry within the seats of learning on Venetian soil. So Galileo was free to teach and make experiments, and because of his teaching the University of Padua began to shine with a reflected glory. Within a few years students not only from Italy but from other lands, among them not a few royal princes, began to flock to Padua to sit at the feet of one who was fast becoming known as the greatest experimental scientist of the day.

It was not until he settled in Padua that Galileo began to specialize in astronomy. His inquiring mind not only busied itself with the motions of bodies on or near the Earth's surface but was exercised also concerning the movements of bodies in the sky. It was only to be expected that one so daring and unconventional would not only get to know of the Copernican system, but would be prejudiced in its favour. Certainly he had become a convinced Copernican as early as 1597. His first letter to Kepler, dated 4 August in that year, is proof of this. Acknowledging the receipt of a presentation copy of Kepler's first book, in which the latter definitely accepted the heliocentric system, Galileo wrote: 'I shall promise to peruse your book dispassionately, and with the conviction that I shall find in it much to admire. This I shall do the more willingly because many years ago I became a convert to the opinions of Copernicus, and by his theory have succeeded in explaining many phenomena which on the contrary hypothesis are altogether inexplicable.'

Galileo's interest in astronomy was further quickened by the new star of 1604, generally known as 'Kepler's star'. This apparition aroused a great deal of interest in scientific and semi-scientific circles, and it is of interest to note that even in those benighted days three extra-mural lectures given by Galileo were attended by great crowds. Galileo, like Tycho and Kepler, theorized on the nature of temporary stars. The absence of parallax proved to him, as to them, that such bodies were situated far beyond the Earth's atmosphere and that the orthodox theory of their nature must be abandoned. Galileo's explanation of new stars was less satisfactory than those of either Tycho or Kepler. He thought they might be vapours of extreme tenuity driven off from the Earth's atmosphere and reflecting the Sun's rays, an hypothesis not really worthy of Galileo's intellectual ability. But at this stage of his career it can hardly be said that Galileo was an astronomer. He was a 'natural philosopher' or, as we might

say, a physicist, who made occasional incursions into the realm of astronomy.

That Galileo's chief fame now rests upon his work in astronomy was due largely to an accidental circumstance. Great results, it has been said, from little causes spring. Some time in 1607 or 1608, a lad apprenticed in the shop of Hans Lippershey, an optician of Middelburg, in Holland, was playing with some of his master's spectacle lenses. Holding two of these in a certain position so that he could see through both, this nameless apprentice noticed that the objects round about him were enlarged and inverted. He mentioned the fact to his master; and Lippershey fixed two of these glasses into a tube, with which he verified his apprentice's observations. He placed the toy in his shop window, where it was seen by a public official, who bought it and presented it to Prince Maurice, the Stadtholder of Holland, who at once realized that the spy-glass might be of some military value.

So runs the traditional account of the invention of the telescope. At all events, the States-General, on 2 October 1608, took into consideration a petition from Lippershey asking for the exclusive right of making and selling such instruments. They politely voted him 900 florins, but they refused to grant his petition. By this time the secret was out, and two other Dutch opticians, Metius of Alkmaar and Jansen of Middelburg, had succeeded in making or designing such instruments.

News travelled slowly in those days, and it was not until June 1609 that reports of the marvellous invention in Holland reached the learned men of Italy. In a letter to his brother-in-law in August 1609, Galileo wrote:

'You must know then that about two months ago (i.e. about June 1609), a report was spread here that in Flanders a spy-glass had been presented to Prince Maurice, so ingeniously constructed that it made the most distant objects appear quite near, so that a man could be seen quite plainly at a distance of 2 miles. This

result seemed to me so extraordinary that it set me thinking, and as it appeared to me that it depended upon the laws of perspective, I reflected on the manner of constructing it, and was at length so entirely successful that I made a spy-glass which far surpasses the report of the Flanders one. As the news had reached Venice that I had made such an instrument, six days ago I was summoned before their Highnesses, the Signoria, and exhibited it to them, to the astonishment of the whole senate. Many of the nobles and senators, although of a great age, mounted more than once to the top of the highest church tower in Venice, in order to see sails and shipping that were so far off that it was two hours before they were seen, without my spy-glass, steering full sail into the harbour; for the effect of my instrument is such that it makes an object 50 miles off appear as large as if it were only five.

'Perceiving of what great utility such an instrument would prove in naval and military operations, and seeing that his Serenity the Doge desired to possess it, I resolved on the 24th inst. to go to the palace and present it as a free gift. On quitting the presence-chamber, I was commanded to bide awhile in the hall of the Senate, whereunto the Procurator, Antonio Prioli, one of the heads of the University of Padua, came, and taking me by the hand, said that the Senate, knowing the way in which I had served it for seventeen years at Padua, and being sensible of my courtesy in making it a present of the spy-glass, had ordered my election (with my good-will) to the Professorship for life, with a salary of 1000 florins yearly; and as there remained yet a year to terminate the period of my last re-election, they willed that the increase of salary should date from that very day.'

The first telescope made by Galileo was apparently of little value, presumably little, if at all, superior to the toys which Lippershey had made. The second telescope, which he presented to the Doge, was shown to the public on 21 August, from the top of the spire of San Marco. A third, a fourth, and then a fifth telescope followed one another from Galileo's workshop in rapid succession, each one better than its predecessor. With the fourth telescope he began to look upwards, and with the fifth, which brought objects thirty

times nearer, Galileo began his epoch-making series of astronomical observations. His first discoveries related to the Moon, and at once brought him into conflict with the Aristotelian philosophers.

'I feel sure', he wrote in his little tract, *The Sidereal Messenger*, in which he announced his discoveries, 'that the surface of the Moon is not perfectly smooth, free from inequalities and exactly spherical, as a large school of philosophers considers with regard to the Moon and the other heavenly bodies, but that, on the contrary, it is full of inequalities, uneven, full of hollows and protuberances, just like the surface of the Earth itself, which is varied by lofty mountains and deep valleys.'

With his fourth telescope Galileo saw the planets as little moons, with appreciable discs, while the stars appeared as points of light only. He found the number of stars invisible to the naked eye 'so numerous as to be almost beyond belief'. 'I had determined', he said, 'to depict the entire constellation of Orion, but I was overwhelmed by the vast quantity of stars and by want of time.' His first observations of the Milky Way were decisive as to its real nature.

'To have got rid of disputes about the Galaxy or Milky Way, and to have made its nature clear to the very senses, not to say to the understanding, seems by no means a matter which ought to be considered of slight importance. . . . The Galaxy is nothing else but a mass of innumerable stars planted together in clusters. Upon whatever part of it you direct your telescope, straightway a vast crowd of stars presents itself to view; many of them are tolerably large and extremely bright, but the number of small ones is quite beyond determination.'

On 7 January 1610 Galileo turned his fifth telescope for the first time to the planet Jupiter, and 'noticed a circumstance' which he had not noticed before, namely three little stars, small but very bright, near to the planet. Two of the stars were to the east of the planet and one to the west. On

the following evening he found to his surprise that all three were on the west side of the planet.

'My surprise', he wrote, 'began to get excited, how Jupiter could one day be found to the east of all the aforesaid fixed stars, when the day before it had been west of two of them; and forthwith I became afraid lest the planet might have moved differently from the calculations of astronomers and so had passed those stars by its own proper motion. I therefore waited for the next night with the most intense longing, but I was disappointed of my hope, for the sky was covered with clouds in every direction.'

By the 10th he was convinced that the 'interchange of position belonged not to Jupiter but to the stars', and on the 13th he saw yet another of these moving stars, making four in all. By this time he was convinced that these stars were not stars at all but small bodies analogous to the Moon, revolving round Jupiter. In announcing his discovery, Galileo was at pains to show how beautifully it dovetailed into the Copernican system.

'We have a notable and splendid argument to remove the scruples of those who can tolerate the revolution of the planets round the Sun in the Copernican system, yet are so disturbed by the motion of one Moon about the Earth, while both accomplish an orbit of a year's length about the Sun that they consider that this theory of the Universe must be upset as impossible; for now we have not one planet only revolving about another, while both traverse a vast orbit about the Sun, but our sense of sight presents to us four satellites circling about Jupiter, while the whole system travels over a mighty orbit about the Sun in the space of about twelve years.'

Probably Galileo made a tactical error in thus emphasizing the significance of his discovery. At all events he soon found that he had stirred up something like the proverbial hornets' nest. The Aristotelian philosophers and the reactionary Churchmen were soon in full cry against him. The more cautious among them believed him to be mistaken; the

less scrupulous made him out to be an impostor. The discovery had upset the *a priori* reasoning of the schoolmen; therefore it must be rejected. There could be only seven 'planets'—the Sun and Moon being accounted as 'planets' or 'wandering stars'. Seven was regarded as a sacred number, and if these four moons existed, the harmony would be broken, for there would be eleven planets. Sizzi, a so-called astronomer of Florence, wrote the following, which, for sheer muddle-headed stupidity, deserves to be immortalized:

> 'There are seven windows given to animals in the domicile of the head, through which the air is admitted to the tabernacle of the body, to enlighten, to warm, and to nourish it. What are these parts of the microcosmos? Two nostrils, two eyes, two ears, and a mouth. So in the heavens, as in a microcosmos, there are two favourable stars, two unpropitious, two luminaries, and Mercury undecided and indifferent. From this and many other similarities in Nature, such as the seven metals, etc., which it were tedious to enumerate, we gather that the number of planets must necessarily be seven. Moreover, these satellites of Jupiter are invisible to the naked eye and therefore can exercise no influence on the Earth, and therefore would be useless, and therefore do not exist. Besides, the Jews and other ancient nations, as well as modern Europeans, have adopted the division of the week into seven days, and have named them after the seven planets. Now, if we increase the number of the planets, this whole and beautiful system falls to the ground.'

Sizzi refused to look through the telescope, doubtless in case he might see the satellites. Clavio, of Rome, went even farther. He declared that he 'laughed at the idea of there being four new planets, to see which they must first be put inside the telescope. Let Galileo keep his opinions, and welcome. I hold to mine.' Another so-called philosopher, Libri, who also refused to look through the telescope, died shortly afterwards, and Galileo, in a letter to a friend, grimly expressed the hope that his stubborn adversary would see the satellites on his way to heaven.

In a letter to Kepler in 1610 Galileo gave forcible expression to his opinion of the orthodox theologico-philosophical party.

'Verily, just as serpents close their ears, so do men close their eyes to the light of truth. To such people philosophy is a kind of book like the *Aeneid* or the *Odyssey*, where the truth is to be sought, not in the Universe or in nature, but (I would use their own words) in comparing texts! How you would laugh if you heard what things the first philosopher of the faculty at Pisa brought against me in the presence of the Grand Duke. He tried hard with logical arguments, as if with magical incantations, to tear down and argue the new planets out of heaven.'

But if these discoveries brought Galileo much abuse and misrepresentation, they also brought him fame. Diplomatically he had named the four new satellites the 'Medicean stars', after the reigning house of his own province of Tuscany; and within a short time the Grand Duke invited him to become 'First Mathematician of the University of Pisa' and 'Philosopher and Mathematician to the Grand Duke'. These posts were largely sinecures. There was no obligation to reside at Pisa or to give any lectures. The offer was therefore a tempting one, and Galileo decided to accept it, and six months after his discovery of Jupiter's moons—12th July 1610—the Grand Ducal decree was issued summoning him to Florence to take up the dual post. There can be little doubt that, in deciding to quit Venetia for Tuscany, Galileo made the grand mistake of his life. Venetia was a republic, and a republic whose rulers were intensely jealous of the Papal power, and the sworn enemies of the Jesuit party. So long as Galileo remained in Padua, he was free to promulgate his views, however heterodox they might be in the eyes of Churchmen and schoolmen; so long as he was a citizen of the Venetian republic, he was safe against all manner of persecution. He did not leave Padua without ample warning of the risk he was taking. But the warnings of his friends were

disregarded and Galileo left Padua in September 1610, after eighteen happy years spent in the service of the University.

Just before he left Padua Galileo turned his telescope to Saturn in the hope of finding one or more moons similar to those of Jupiter. To his surprise he found Saturn to be a 'triple' star.

'I have observed', he announced in November 1610, 'with great admiration that Saturn is not a single star but three together, which, as it were, touch each other. They have no relative motion . . . the middle being much larger than the lateral ones. If we examine them with a glass of inferior power, the three stars do not appear very distinctly. Saturn has an oblong appearance, somewhat like an olive, but by employing a glass which multiplies the superficies more than 1000 times, the three globes will be seen very distinctly and almost touching, with only a small dark space between them. I have already discovered a court for Jupiter, and now there are two attendants for this old man, who aid his steps and never leave his side.'

In the course of the next two years Galileo was much perturbed by the fact that the two stars were growing gradually smaller, and in the end of 1612 he was amazed to find that they disappeared altogether. This put him into a great state of apprehension as to the exultation of the Aristotelians, who would be sure to dub him the victim of illusion. In a letter to a German friend, Welser, a merchant of Augsburg, he wrote:

'Looking at Saturn within these last few days, I found it solitary without its accustomed stars, and, in short, perfectly round and defined like Jupiter, and such it still remains! Now what can be said of so strange a metamorphosis? Are, perhaps, the two smaller stars consumed like spots on the Sun? Have they suddenly vanished and fled? Or has Saturn devoured his own children? Or was the appearance, indeed, fraud and illusion, with which the glasses have for so long mocked me and many others who have observed with me? Now, perhaps, the time is come to revive the withering hopes of those who, guided by more profound contem-

plation, have fathomed all the fallacies of the new observations, and recognized their impossibility. I cannot resolve what to say in a change so strange, so new, so unexpected. The shortness of time, the unexampled occurrence, the weakness of my intellect, the terror of being mistaken, have greatly confounded me.'

Five years later Galileo noted the attendant bodies again in the form of 'handles', but he was quite unable to find any solution of the problem.

A month after his arrival in Florence, Galileo detected the phases of Venus. 'Venus', he announced, 'rivals the phases of the Moon; for Venus being now arrived at that part of her orbit in which she is between the Earth and the Sun, and with only a part of her enlightened surface turned towards us, the telescope shows her in a crescent form, like the Moon in a similar position.' Quite obviously this discovery still further confirmed the Copernican system—though it might have been reconciled with the Tychonic. Of Mercury Galileo was able to make little or nothing; while as to Mars he could detect nothing on its surface, but he strongly—and rightly—suspected a slight phase similar to that of the Moon a few days before the full.

Even more sensational was his discovery of sun-spots early in 1611. He was not the sole discoverer of these objects. Claims for at least independent discovery have been made for an Englishman, Harriot; a German, Scheiner; and a Dutchman, Fabricius. And there appears little reason to doubt that in the case of the Dutchman the discovery was quite independent of Galileo. But it was Galileo's announcement that stirred the wrath of the Aristotelians. Aristotle had declared the Sun to be without spot or blemish, and of course, Aristotle was right; so Galileo must be wrong. Nevertheless, Galileo pursued his observations, detecting not only the spots but the brighter regions called faculae, and rightly surmising that the spots were not little planets transiting the Sun's

disc as Scheiner had supposed, but were definitely affixed to the Sun itself.

While these discoveries were denounced by the more extreme Aristotelians and by the more ignorant representatives of the priesthood, the ecclesiastical authorities had not passed any comment on them. At length, in 1611, Galileo went to Rome, where he met the Pope (Paul V), and was introduced to Cardinal Barberini, who was later to ascend the papal throne. Through one of his telescopes the chief dignitaries saw with their own eyes all that he had discovered and he was received with the greatest cordiality.

But, as his Venetian friends had foreseen, a conflict was bound to eventuate between ecclesiasticism and science, and the more enlightened churchmen were to have their hands forced by the extremists and the die-hards. 'I foresee', wrote Fra Paolo Sarpi of Venice, a staunch friend of Galileo, 'that the ecclesiastical authorities will soon change a question of physics and astronomy into one of theology.' This forecast turned out to be correct. The reactionaries created an agitation which resulted in Cardinal Bellarmine summoning Galileo and 'admonishing' him to abandon the Copernican system. Galileo appears to have simply heard Bellarmine's statement and request without making any definite promise of recantation. At the same time, the work of Copernicus was declared 'suspended' in order to be corrected. As the desired corrections could not be made by any one, the book was on the Index of prohibited works for two centuries. In 1630 Galileo decided to publish his important book *A Dialogue on the Two Principal Systems of the World*. There can be no doubt that in publishing this book, in which the Copernican system was to all intents and purposes defended, he was running a real risk; but there had been changes at the Vatican. His friend Cardinal Barberini had become Pope, with the title of Urban VIII, and he had declared even after his elevation to the papal chair that if he could have helped it

the decree of condemnation on the Copernican system would never have been passed. His friend and disciple Castelli had been appointed mathematician to the Pope, and was on terms of intimate friendship with the Pope's relatives. However, Galileo took nothing for granted, and agreed to submit his manuscript to the authorities; further, he had a long interview with his friend the Pope.

The book accordingly appeared, and neither Galileo nor his friends anticipated any further trouble. Great was their astonishment when, in August 1632, further sale was prohibited and a Commission appointed to examine it. There can be little doubt as to the cause of the Pope's change of front. The character in the dialogue who defended the Ptolemaic system had been given the name of 'Simplicio'; and Galileo's enemies had succeeded in convincing the Pope that Simplicio—'the simpleton'—was intended to represent him, and accordingly he treated this as a mortal insult. The Commission, which consisted of men who knew nothing of mathematics, or indeed of science of any kind, reported unfavourably on the book, and condemned Galileo for 'deviating from the hypothetical standpoint, by maintaining decidedly that the Earth moves, and that the Sun is stationary'; and this verdict was given in spite of the fact that the book had been revised twice, and that all the conditions of the censorship had been complied with. The result of the report was the appearance of Galileo before the Inquisition. The astronomer was now in his seventieth year, and the victim of many bodily ailments, and this perhaps explains his weakness during the trial. It was indeed a tragic climax to a great career. On 22 June 1633, in the large hall of the Dominican Convent of Santa Maria Sofia Minerva at Rome, in presence of seven cardinals, he was compelled in the course of a long and humiliating declaration to say:

'I abjure, curse, and detest the said errors and heresies and

generally every error and sect contrary to the said Holy Church; and I swear that I will nevermore in future say or assert anything verbally or in writing which may give rise to a similar suspicion of me; but that if I shall know any heretic or any one suspected of heresy, I will denounce him to this Holy Office or to the Inquisitor and Ordinary of the place in which I may be.'

One of Galileo's biographers has contended that he was justified in committing what amounted to perjury on the specious plea that 'revealed truth may require its martyrs . . . but scientific truth certainly requires none'. But surely truth is truth and cannot be divided up as Mr. Fahie suggests; and perjury is always perjury, whether or not the subject under discussion be scientific or religious. It would seem that Brewster's contention is unanswerable: 'Had Galileo but added the courage of the martyr to the wisdom of the sage, had he carried the glance of his indignant eye round the circle of his judges, had he lifted his hands to heaven and called on the living God to witness the truth and immutability of his opinions, the bigotry of his enemies would have been disarmed, and science would have enjoyed a memorable triumph.' Probably the bigotry of his enemies would have sent him to the stake, but he would have been spared eight unhappy years, and would have passed out of life with his name unsullied; and science would, indeed, have enjoyed a memorable triumph. The victory was with science in any case; it would have been a more glorious victory had Galileo stood true to his innermost convictions.

Galileo was now a broken man. He was close on seventy; he had endured various ailments for years, and these had been accentuated by his sufferings, mental and bodily. A supreme sorrow descended upon him just after his return to his own home at Arcetri, near Florence. His elder daughter, a nun in the convent of San Matteo, a young woman of sweet disposition and keen intelligence, to whom her father was devotedly attached, died at the age of thirty-three.

Galileo's cup of affliction was now full and running over. 'I feel myself', he wrote, 'perpetually called by my beloved daughter.'

Nevertheless, Galileo's career was not yet at an end. He only occasionally made astronomical observations, for his sight was now failing. He now devoted himself chiefly to dynamics, and in 1636 he completed his *Dialogues on the Two New Sciences*. It was impossible, owing to papal prohibition, to have the book printed in Italy, and it was not until 1638 that it issued from the press of a printing-house in Leyden. In this book Galileo summed up the work of a lifetime on motion, acceleration, and gravity. As Lagrange said long afterwards, philosophers before Galileo 'considered the forces which act on bodies in a state of equilibrium only, and although they could only attribute in a vague way the acceleration of heavy bodies, and the curvilinear movement of projectiles, to the constant action of gravity, nobody had yet succeeded in determining the laws of these daily phenomena on the basis of a cause so simple. Galileo made the first important steps.' The three laws of motion enumerated by Newton in 1687 were in the main based on Galileo's work. These laws are that (1) every body continues in its state of rest or of uniform motion in a straight line, except in so far as it may be compelled by force applied to it to change that state; (2) change of motion is proportional to the applied force, and takes place in the direction in which the force acts; and (3) to every action there is always an equal and contrary reaction, or the mutual actions of any two bodies are always equal and oppositely directed. The first two of these laws were implicit in Galileo's work.

Before this his last work had been published, Galileo's sight failed altogether, and he became totally blind. Writing to his friend Diodati of Pisa, he said:

'Alas, sir, Galileo, your devoted friend and servant, has been for a month totally and incurably blind, so that this heaven, this earth,

this universe, which with wonderful observations I had enlarged a hundred, a thousand times beyond the belief of bygone ages, henceforth for me is shrunk into the narrow space which I myself fill in it. So it pleases God; it shall therefore please me also.'

As Castelli, Galileo's friend and disciple, finely said:

"The noblest eye which nature ever made is darkened—an eye so privileged and so gifted with rare qualities that it may with truth be said to have seen more than the eyes of all who are gone and to have opened the eyes of all who are to come.'

After three years of increasing enfeeblement, blind and sad and lonely, kept under strict surveillance by the Inquisition, virtually a prisoner in his own home, Galileo died at his villa at Arcetri, 8 January 1642, in his seventy-eighth year. The ecclesiastical authorities succeeded in vetoing the project for a public funeral, a funeral oration, and a monument, and he was quietly buried in the Chapel of the Novices in the Church of Santa Croce in Florence. Not for half a century after his death did his disciple Viviani venture to erect a memorial to the great astronomer, and not for nearly two hundred years was Galileo's *Dialogue*, along with the work of Copernicus, removed from the Index of prohibited books. But great was the truth, and it prevailed.

When the life of Galileo was ebbing away in blindness and loneliness in his Florentine villa, a bright boy was experimenting with machines and mechanical models in his father's home in The Hague. This was **Christian Huyghens**. Born at The Hague on 14 April 1629, Christian was the second son of Constantine Huyghens, *litterateur* and statesman, one of the confidants of the Prince of Orange. His older brother Constantine, with whom he had many ties of common interest, followed in his father's footsteps and became a noted politician. In time he was appointed secretary to that Prince of Orange who became, as a result of the Revolution, William III of Great Britain.

The younger of the two boys did not follow his father

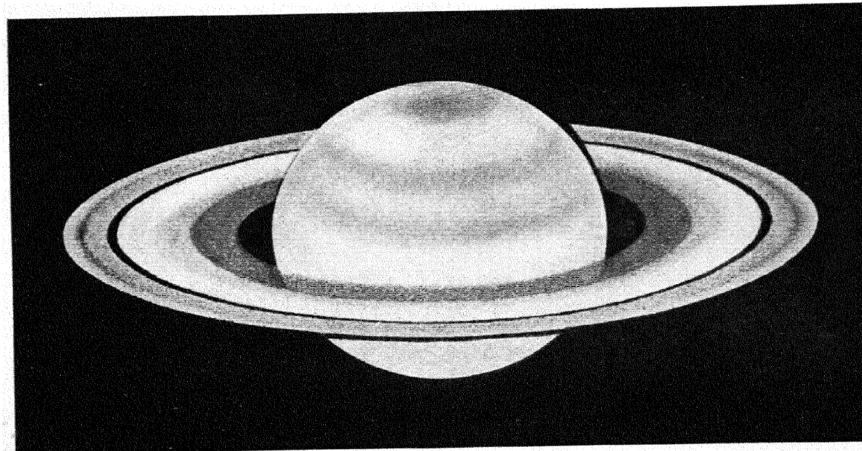
into the realm of politics, but concentrated on the sciences. His bent for mathematics and mechanics manifested itself as early as his thirteenth year. At an early age he was enrolled at the University of Leyden. The idea of his father was to make him a lawyer, but the young mathematician showed no aptitude for this study, and at last he got his way. By the age of twenty-two he was an astronomer, and by twenty-four he had several treatises on geometry to his credit. He was still a young man when he commenced that series of telescopic observations which has made his name famous in the history of astronomy. At the age of twenty-five he began to experiment in telescope-making. His brother assisted him, and between them they succeeded in making lenses much more powerful and efficient than those used by Galileo. The telescopes made by Huyghens look primitive enough to the eyes of to-day. They were absurdly long; and those who saw them at the Huyghens Tercentenary Exhibition in the Leyden Observatory could not but smile at the long thin tubes fastened along the whole length of the wall of the room used as the Huyghens museum. Nevertheless, with those instruments some discoveries of first-class importance were made.

First of all, on 25 March 1655, came the detection of the largest and brightest of Saturn's satellites, Titan. Had he searched more closely, he could easily have discovered one, and perhaps two or three, of the fainter ones which Cassini picked up some years later. But, like Copernicus and Kepler, he had a strain of the medievalist in him, and he conceived the idea that Saturn could only have one moon. He reasoned thus: There are six planets—Mercury, Venus, Earth, Mars, Jupiter, and Saturn. There are now six satellites known—four Jovian, one terrestrial, one Saturnian. Therefore the Solar System is complete! No need to search for more moons! Strange reasoning for so great a man.

Saturn, as we all know, had sorely tried the patience of

Galileo. The apparent changes in the planet's shape seemed to baffle him completely, and he could not formulate a plausible hypothesis. The more powerful telescope of Huyghens turned on Saturn in 1655–6 was the means of solving the mystery of the strange shape of the planet and its stranger variation. 'I came to understand', he said, 'that inasmuch as the circuit of Saturn and the adhering bodies was so short, this could happen only under no other condition than that the globe of Saturn were assumed to be surrounded equally on all sides by another body, and that thus a kind of ring encircled it about the middle.' In March 1656 Huyghens, practically certain that he had hit on the true explanation, put forth his hypothesis in what he called 'confused letters'. These were—'a a a a a a a c c c c c d e e e e e g h i i i i i i i l l l l m m n n n n n n n n n o o o o p p q r r s t t t t t u u u u u.' This, which was in harmony with the recognized practice of the time, was done in order to conserve the rights of discovery. We would not do this kind of thing now. It suggests 'having it both ways'—'heads, I win; tails, you lose.' If the supposed discovery were disproved, nothing further was heard of it, and the observer did not suffer in prestige; if, on the other hand, it were confirmed, then the credit went to him who first put out the anagram. Anyhow, Huyghens had three years in which to test his discovery, and in 1659 he arranged the letters in their proper order into the Latin sentence: 'Annulo cingitur, tenui, plano, nusquam cohaerente, ad eclipticam inclinato.'—'The planet is surrounded by a thin flat ring, nowhere touching it, and inclined to the elliptic.'

Huyghens was about thirty years of age at the time of this his greatest discovery. In the same year, 1659, he commenced to study Mars. Galileo, as we know, was able to make little of our second nearest neighbour. His tiny 'optic tube' was inadequate. All that he could achieve was to suspect strongly the existence of a slight Martian phase at quadrature, in accordance with the Copernican system. Fontana of Naples



SATURN AND ITS RINGS

in 1638 succeeded in catching fleeting glimpses of dusky markings on the little red disc. But Huyghens was the first to make a drawing of one of these. This was the Syrtis Major, drawn by him on 28 November 1659. Watching this marking carefully, he concluded that the rotation of Mars was performed in about twenty-four hours. Seven years later, Cassini determined the Martian day as 24 hours 40 minutes long—a remarkable approximation to the truth. Huyghens thought that 'the land in Mars is of a blacker hue than that of Jupiter or the Moon, which is the reason of his appearing of a copper colour and his reflecting a weaker light than is proportionate to his distance from the Sun', which was another way of saying that the planet's albedo was lower than that of Jupiter. Huyghens erred, however, in thinking he had evidence that the axis of Mars is perpendicular to the plane of its orbit. 'The inhabitants have no perceivable difference between summer and winter, the axis of that planet having very little or no inclination to his orbit, as has been discovered by the motion of his spots.' Huyghens evidently believed this until the end of his life, for these words occur in his posthumous book, of which I shall have more to say.

Among the other discoveries in this fruitful period of Huyghens's youthful research was that of the Orion nebula.

> 'In the sword of Orion are three stars quite close together. In 1656, as I chanced to be viewing the middle one of these with the telescope, instead of a single star twelve showed themselves (a not uncommon occurrence). Three of these almost touched each other, and with four others shone through a nebula, so that the space around them seemed far brighter than the rest of the heavens, which was entirely clear and appeared quite black, the effect being that of an opening in the sky through which a brighter region was visible.'

In 1659, the year of the announcement of his discovery of Saturn's ring, Huyghens presented the first 'pendulum clock' to the States-General of Holland. The vast importance of

this for the future of astronomy can hardly be over-estimated. His reputation now became European. He visited England in 1660, and was elected to the Royal Society in 1663. In 1665 Louis XIV, always on the look-out for learned foreigners who might bring more lustre to the name of France, invited him to settle in Paris. He remained in France for sixteen years, and during this period of his activity he enunciated the 'wave-theory' of light. In 1681 the renewed persecution of the Protestants, which culminated in the revocation of the Edict of Nantes, put an end to his residence in France. He returned to Holland, where he continued his astronomical work, constructing still longer telescopes and devising the Huyghenian eyepiece. He died at The Hague, the city of his birth, on 8 June 1695, in his sixty-seventh year.

Apart from his *Systema Saturna*, and a number of mathematical and physical treatises, Huyghens' best-known work is probably the *Cosmotheoros*, published at The Hague in 1698, three years after his death. In this book he gave his latest views on the Solar System and the outside Universe, with special reference to the physical state of the other planets and the possibility of a plurality of worlds. He had just finished writing the book when he fell mortally ill, and he begged his brother Constantine, then in England, to see to its publication. Sir David Brewster stated that 'this interesting treatise has never been translated into English'. This appears to be erroneous. A work entitled '*The Celestial Worlds Discovered, or Conjectures concerning the Inhabitants, Plants and Productions of the Worlds in the Planets*, written in Latin by Christianus Huyghens and inscribed to his brother Constantine Huyghens, late Secretary to his Majesty King William,' is certainly *Cosmotheoros* translated into English.

This book of Huyghens gives us a tolerably clear idea of the extent of man's knowledge concerning the worlds outside of the Earth at the close of the seventeenth century. Of the Moon Huyghens wrote thus: 'The surface of the Moon is

found . . . to be diversified with long tracts of mountains and again with broad valleys. For in those parts opposite to the Sun, you may see the shadows of the mountains and often discover the little round valleys between them with a hillock or two perhaps rising out of them.' Huyghens went on to say that he could not 'see anything like sea there'.

'For those vast countries which appear darker than the other, commonly taken for and called by the name of seas, are discovered with a good long telescope to be full of little round cavities, whose shadow falling within themselves makes them appear of that colour, and those large champains there in the Moon you will find not to be always even and smooth, if you look carefully upon them. Neither of which two things can agree to the sea. . . . Nor do I believe that there are any rivers, for if there were they would never escape our sight, especially if they run between the hills as ours do. Nor have they any clouds to furnish the rivers with water. For if they had, we should sometimes see one part of the Moon darkened by them and sometimes another. Whereas we have always the same prospect of her. It is certain, moreover, that the Moon has no air or atmosphere surrounding it as we have.'

Huyghens estimated the distance of Sirius, which he presumed to be the nearest star, as 27,664 times that of the Sun. And he took the step, which Kepler and even Galileo hesitated to take, of making the Sun merely one star among others. Referring to Kepler's idea of the sphere of fixed stars as two German miles in thickness, Huyghens wrote:

'A mere fancy without any shadow of reason. I cannot but wonder how such things as these could fall from so ingenious a man and so great an astronomer. But I must give my vote with all the greatest philosophers of our age to have the Sun of the same nature as the fixed stars. And this will give us a greater idea of the world than all those other opinions. For then why may not every one of these stars or suns have as great a retinue as our Sun, or planets with their moons to wait upon them?'

He thought the stars to be innumerable.

'For if with our bare eye we can observe above a thousand, and with a telescope can discover ten or twenty times as many, what bounds of number must we set to those which are out of the reach even of these assistances, especially if we consider the infinite power of God? Really, when I have been reflecting thus with myself, methought all our arithmetic was nothing, and we are versed but in the very rudiments of numbers, in comparison of this great sum. For this requires an immense treasury, not of twenty or thirty figures only, but of as many as there are grains of sand upon the shore. And yet, who can say that even this number exceeds that of the fixed stars? . . . Indeed, it seems to me certain that the Universe is infinitely extended, but what God has been pleased to place beyond the region of the stars is as much above our knowledge as it is our habitation.'

And from this cosmological concept he deduced the plurality of worlds. 'What a wonderful and dazzling scheme have we here of the magnificent vastness of the Universe? So many suns, so many earths, and every one of them stocked with so many herbs, trees and animals, and adorned with so many seas and mountains! And how must our wonder and admiration be increased when we consider the prodigious distance and multitude of the stars!'

II

ISAAC NEWTON

'Nature and Nature's laws lay hid in night,
God said, Let Newton be! and all was light.'

So wrote Alexander Pope, the eighteenth-century poet. We must, of course, allow for what is called the poet's licence. It would be more correct to say that when Newton came, the dawn broke into morning. Galileo and Kepler had thrown much light on the working of the world. The former had investigated the motions of bodies on or near the Earth's surface, the latter the movements of bodies in the sky. It was Newton's task to effect a synthesis of the results attained by those who preceded him.

Isaac Newton was born at Woolsthorpe, near Grantham, in Lincolnshire, on 25 December 1642. He came of a farming family settled for at least two generations in Lincolnshire. There is a tradition to the effect that the family was of Scottish extraction, and originally hailed from East Lothian. A friend of the famous James Gregory passed on to the equally famous Dr. Thomas Reid the story that Newton in middle life had told the former that he believed his grandfather to have been a native of East Lothian, and to have been one of the many Scots who went up to London to seek fame and fortune, and found neither, at the time of the Union of the Crowns. But the story is of very doubtful authenticity and it is more likely that the family was of pure English extraction.

Newton was a very delicate child, and in his early infancy was scarcely expected to survive. He was an only child, and his father had died before his birth. When he was three years of age his mother married again, her second husband being the rector of a neighbouring parish. After her removal to the nearby rectory, her place at Woolsthorpe was taken by her mother, who superintended the upbringing of the little Isaac. In early boyhood Isaac seems to have outgrown all the

delicacies of his infancy, and he was sent to small schools in the neighbouring villages of Stoke and Skillington. By the time he had reached the age of twelve he was too far advanced for these parochial seats of learning and was enrolled at the King's School, Grantham, in which town he lodged with an apothecary of the name of Clark. Here he remained for several years.

At this stage in his career Newton seems to have been in no way remarkable. He did not excel in his lessons; and he did not evince the least interest in the physical side of school life, which so often interests those lads to whom the drudgery of learning is distasteful. Brewster records that young Newton was awakened from his intellectual lethargy by the brutality of the boy just above him in class. This boy gave him a severe kick, and Newton, who was not able to master him physically, decided to humiliate him intellectually. He began to apply himself to study, with the result that he not only outstripped this particular bully, but rose to the top of the class. His intellectual powers once awakened did not go to sleep again. He now began to spend his leisure in making mechanical toys, and soon provided himself with saws, hatchets, hammers, and all kinds of tools. Among the toys which he constructed in his spare time were a windmill, a moving carriage, and a water-clock. The latter toy was made out of a box which he procured from the brother of his landlady. Brewster recorded that it 'was about four feet high and of a proportional breadth, somewhat like a common house-clock. The index of the dial plate was turned by a piece of wood which either fell or rose by the action of dropping water. As it stood in his own bedroom, he supplied it every morning with the requisite quantity of water, and it was used as a clock by Mr. Clark's family, and remained in the house long after its inventor had quitted Grantham.' He seems to have specialized in the making and flying of kites, and it is on record that he used to attach paper lanterns to the kites, and so in the dark mornings frighten the simple country

folks into thinking that they were being visited by those terrible celestial bodies called comets. Even at this early age Newton was studying the heavenly bodies and had constructed a number of sun-dials, one of which still exists in the neighbouring village church of Colsterworth.

At the age of fourteen Newton left school and returned to Woolsthorpe. His mother was now a widow for the second time, and she naturally desired her son to qualify for the oversight of the farm. Accordingly, Newton spent the two years from 1656 to 1658 trying to learn the rudiments of agriculture; but he never got beyond trying to learn. Frequently he was sent to Grantham with a servant to accustom him to the buying and selling of corn; but the way in which he comported himself on these occasions did not give his mother much encouragement.

'An old trustworthy servant', according to Brewster, 'generally accompanied him on those errands. The Inn which they patronized was the Saracen's Head at West Gate; but no sooner had they put up their horses than our young philosopher deserted his commercial concerns and betook himself to his former lodgings in the apothecary's garret, where a number of Mr. Clark's old books afforded him abundance of entertainment till his aged guardian had executed the family commissions and announced to him the necessity of returning. At other times, he deserted his duties at an earlier stage, and entrenched himself under a hedge by the wayside, where he continued his studies till the servant returned from Grantham. The more immediate affairs of the farm were not more prosperous under his management than would have been his marketings at Grantham. The perusal of a book, the execution of a model, or the superintendence of a water-wheel of his own construction, whirling the glittering spray from some neighbouring stream, absorbed all his thoughts; while the sheep were going astray and the cattle were devouring or treading down the corn.'

It is recorded that, during the violent storm which synchronized with the death of Oliver Cromwell, young Isaac spent his time jumping with and against the gale in an

endeavour to estimate its force, instead of seeking to minimize its disastrous consequences. Accordingly, his mother and his uncles reluctantly concluded that he was not made to be a farmer; and to his great delight they decided to send him back to Grantham, with a view to preparing to enter the University of Cambridge.

After two years of preparation Newton entered Trinity College, Cambridge, on 5 June 1661. The earlier part of his career there did not give much promise of his future intellectual pre-eminence. He was solid, but not brilliant. Indeed, as late as 1664, the examiners for a scholarship for which he competed commented on his slender knowledge of Euclid. He soon made good, however; and while still a student he mastered several important contemporary works, including Kepler's *Optics*, which exercised a powerful influence over him and directed his attention to optical phenomena. In 1665 he took his degree, but it does not appear that he held a specially brilliant place in the final awards. He was destined, however, for a Fellowship of Trinity, but before entering on his duties the outbreak of the Great Plague at Cambridge necessitated the closing of the University, and led to his return to Woolsthorpe, where during an enforced exile he was to give his undivided attention to a problem which had now gripped him—the system of the world.

Apparently Newton was never anything else than a Copernican. He took the heliocentric system and Kepler's laws and Galileo's discoveries all for granted. At the early age of twenty-four his mind was exercised by the supreme problem of why the Copernican system was true, why the smaller bodies, the planets, moved round the larger body, the Sun. We are all familiar with the story of the apple which fell in the garden at Woolsthorpe during the autumn of 1666. According to one version of the story, Newton was sitting in the garden when the apple fell; according to another, he was looking through a window on the first floor of his mother's

house. Only the most ignorant persons, of course, believe that the fall of the apple led Newton to the discovery of gravity. That some kind of gravitational force existed, pulling all things to the centre of the Earth, had been known from earliest times, and Galileo had shown in his statements of the laws of motion how this force acts. Whether the story be true or not, the fall of the apple did not lead Newton to discover gravity; what it did, most likely, was to set him thinking, and Professor Brodetsky, one of his later biographers, has so well outlined the probable train of thought in Newton's mind that I make no apology for quoting his words here:

'Why do the planets go round the Sun? Why do they not move in straight lines? Evidently there is a force pulling them out of the straight path at every moment, and clearly this force is due to the Sun. The Moon goes round the Earth, and does not go in a straight line. This must be due to the Earth. Ah! an apple has just fallen to the ground; the Earth has pulled it down. How far up does the Earth's influence extend? We know that no matter how high up we go—to the summits of the highest mountains—this force exists without obvious weakening. Does the Earth's gravitation extend to any distance, no matter how great,—perhaps even as far as the Moon? Can this be the force that compels the Moon to accompany the Earth, to travel round and round the Earth indefinitely as the Earth travels round the Sun? Yes, this is a pretty theory; can it be proved? Can it be shown that the pull required to explain the Moon's motion is just that afforded by the Earth's gravitation? Any attempt at such proof must postulate some law according to which the gravitative pull of the Earth varies with the distance from the Earth; for clearly we cannot suppose this pull to be the same for all distances, even to the ends of the Universe. It must diminish as the distance increases. What is the law of this diminution?'

Once Newton's mind had got on to this hopeful track, a great step forward had to be taken. There was much to do, however—much hard calculating. It was necessary to determine, first of all, how the pull of gravity would vary with the

distance from the attracting body; and secondly, to ascertain whether the pull exerted by the Earth on the apple was the same as that exerted on the Moon.

Newton's line of attack on the problem was in the first instance by way of a study of the motions of the planets round the Sun. Kepler had put forward the idea that the planets were influenced by some force resident in the Sun; but this idea was vague in the extreme. Newton took a great step forward. Even at this early stage he satisfied himself that the motions of the planets would be explained as due to the action of the Sun, if the Sun is assumed to be capable of producing in any given planet an acceleration towards itself which is proportional to the inverse square of that planet's distance. This means that at twice the distance it is a quarter as great, and at three times the distance one-ninth as great. He next sought to ascertain whether the Moon's motion round the Earth could be similarly explained. But in this investigation he had to take account of a factor which he had been able to neglect in his discussion of the planetary motions. One of his biographers has put it thus:

'The distances of the planets round the Sun being large compared with the size of the Sun, it makes little difference whether the planetary distances are measured from the centre of the Sun or from any other point on it. The same is true of the Moon and Earth, but when we are comparing the action of the Earth on the Moon with that on a stone situated on or near the ground, it is clearly of the utmost importance to decide whether the distance of the stone is to be measured from the nearest point of the Earth, a few feet off, from the centre of the Earth 4,000 miles off, or from some other point. Provisionally at any rate, Newton decided on measuring from the centre of the Earth.'

The successful outcome of Newton's investigation depended on an accurate measure of the radius of the Earth. Fairly accurate measures of this quantity had been made in the middle of the seventeenth century. But in his country

home at Woolsthorpe Newton did not have access to the results of these investigations. He took the radius to be 3,440 miles, which was too small, and consequently his result was seriously affected. If the force pulling the Moon were the same as that pulling the stone—on the basis of this measure of the radius—the acceleration of the Moon to the Earth should be 0·00775 feet per second added each second. But on Kepler's laws the actual figure was 0·00895 feet. The discrepancy was so great that Newton appears to have concluded that he was on the wrong track. So he laid aside the investigation, and turned to other branches of science.

The peril of the plague having passed, Cambridge University reopened its doors and Newton returned to Trinity College in March 1667. On 1 October of that year he was elected to a minor Fellowship, and in the following year to a major Fellowship. He was thus in a position to devote himself to scientific work. By 1669 his abilities as a mathematician had become so evident that he was, on 29 October of that year, chosen to fill the Lucasian chair of Mathematics. He was very young for a professor—even in those days—only twenty-seven years of age. His duties were not onerous; they included a weekly lecture during one term each year on some branch of mathematics, and also two hours a week of private tuition with students who might desire to consult him. The branch of mathematics on which he decided to lecture was optics, and to this branch he was to devote his chief energies for several years to come.

His earliest optical studies synchronized with his apparently abortive work on gravitation. Early in 1666 he procured a prism and made the first scientific study of the dispersion of light. He may be called, indeed, the discoverer of the spectrum. Refraction through a prism, he found, disperses a beam of white light into the primary colours—red, orange, yellow, green, blue, and violet: he likewise showed that the coloured patch of light could be made white again—and

circular as well—by using another exactly similar prism, but with the bending taking place in an opposite direction. This was indeed, as he afterwards claimed, 'a considerable detection into the operations of nature'.

This led on to his study of the telescope and to his invention of the reflector. By the 'sixties of the seventeenth century the development of the telescope seemed to have come to a full stop. Astronomers found that, as larger instruments were constructed, an unpleasant effect known as chromatic aberration became evident. The images of stars and planets were not clear and distinct, but surrounded by fringes of colour, and it was evidently a matter of very great difficulty, if not, indeed, quite impossible, to eliminate this effect. Newton concluded—rightly in respect of telescopes as then made—that elimination was impossible. Refraction meant the breaking up of a beam of light in greater or less degree; and so Newton decided that the refracting telescope could not be improved. It was not until the following century that a method of correcting this aberration was devised.

Newton thus entered on a line of research and experiment which resulted in the construction of the first reflecting telescope. True, the idea of such an instrument first occurred to a brilliant Scotsman, James Gregory, the first of a family of distinguished men who came to be known as 'the academic Gregories'. Gregory conceived the idea of putting a mirror at the end of a tube and making a small round hole in it, into which an eyepiece was fitted. The open end of the tube was to be turned on a star or planet, and the image reflected to a small mirror, which in turn reflected it into the eyepiece. This style of instrument, known as the Gregorian, had a certain vogue in the following century. Newton rightly objected to the hole in the large mirror, which resulted in a serious loss of light. Accordingly, he set himself to devise a different place for the secondary mirror. In his design this second mirror was placed at an angle of 45 degrees with the

axis of the telescope, and the rays were reflected back from this second mirror, through a hole in the side of the telescope tube, where the eyepiece was fitted in.

While Gregory merely devised an instrument, Newton constructed one. The first reflector ever made, 6 inches long and 1 inch in aperture, was completed by him at the end of 1668. With this telescope he saw Jupiter as a little moon, with its four satellites beside it, and he discerned the phases of Venus with some difficulty, which would seem to indicate that it was not quite so good as the instrument with which Galileo discovered these phases. Dissatisfied with this telescope, Newton in 1671 set about constructing another, which he regarded as somewhat better. The recently founded Royal Society, having come to hear of his invention, asked him to send it up to London for inspection, a request to which he gladly agreed. One result of the inspection was Newton's election as a Fellow of the Society on 11 January 1672. His first paper, on the discovery of the spectrum and the analysis of colour, was read to the Society about three weeks later. The little telescope which led to his election to the Royal Society is still preserved in the Society's library, with the following inscription: 'Invented by Sir Isaac Newton and made with his own hands, 1671.'

During this period of his life Newton lived a quiet life in Trinity College, Cambridge. About the middle thirties of his career he appears to have developed those habits of eccentricity and absent-mindedness of which so many stories have been told. His absorption in mental work of great difficulty rendered him careless as to food and sleep. 'His breakfast', we are told, 'was orange peel boiled in water, which he drank as tea, sweetened with sugar, and with bread and butter.' This mode of living induced debility, which in turn led to hypochondria; indeed at one time during this period he believed himself to be sinking into consumption.

According to the regulations of Cambridge University,

when a College Fellowship expired, the Fellow could only be re-elected if he consented to go into holy orders—a pernicious rule which encouraged intellectual dishonesty on the part of scholars and ecclesiastical professionalism on the part of those thus coerced into taking orders. Newton was resolved not to enter the Church. He was a profoundly religious man, but he claimed the right of independent judgement in theological as well as scientific matters, and declined to be fettered by creeds. Further, he had no intention of taking orders when there was no prospect of his entering the ministry. Had he been deprived of his Fellowship he would have had to live on the salary of his professorship, which was by no means princely. He therefore petitioned the king for a dispensation to allow him to retain the Fellowship as a layman, and in virtue of his professorship. Charles II was not a king to whom posterity has much reason to be grateful, but his decision to grant this petition is something to his credit.

In the 'seventies of the century Newton returned to the problems of gravitation and motion which had exercised his mind at an earlier stage. In 1673 he was in communication with Huyghens on gravity, and in 1675 he wrote to Mercator, the famous geographer, explaining in a satisfactory way the libration of the Moon discovered by Galileo as due to the combination of uniform motion of the Moon on its axis and irregular motion in its orbit. The celestial motions continued to interest him, although he had made no further frontal attack on the general problem. But in 1679 his interest was stimulated on receipt of a letter from Robert Hooke, a man of great intellectual power, indeed of genius, whose unfortunate habit of dissipating his energies over too wide a scientific field probably robbed him of the discovery which afterwards fell to Newton. In his communication Hooke made the statement that in the case of a projectile the curve described by it would be an ellipse if the Earth's gravity varied inversely as the square of the distance. This, and the

speculations of others, set Newton thinking, and eventually induced him to take up the threads of his study of gravity where he dropped them in 1666. At first he dissented from Hooke's statement that a projectile following the inverse square law would describe an ellipse. He believed it would follow a spiral curve. Soon afterwards, however, he saw that Hooke was probably right, although he had given no definite proof; and as a result of his own calculations he found that Hooke was certainly right.

> 'Though his correcting my spiral', said Newton, 'occasioned my finding the theorem by which I afterwards examined the ellipsis: yet am I not beholden to him for any light into the business, but only for the diversion he gave me from my other studies to think on these things, and for his dogmaticalness in writing, as if he had found the motion in the ellipsis which inclined me to try it after I saw by what method it was to be done.'

The relations between these two distinguished men were very strained. Hooke later laid claim to be the real discoverer of the law of gravitation, which claim Newton stoutly resisted.

But this study of the path of a projectile did not at once bring Newton back to the study of gravity. He was engrossed in his work on light, and he was still under the impression that he could not proceed farther along the path which he had trod at Woolsthorpe. The discrepancy due to the inaccurate measure of the radius of the Earth which he adopted was the stumbling-block. In 1672, however, the French astronomer Picard succeeded in getting an accurate measure of the Earth's radius, and showed that the earlier measures had been 15 per cent. in error. Newton must have been aware of this, for Picard's results were communicated to the Royal Society at the very meeting at which Newton was elected a Fellow. Yet he allowed ten years to elapse before he corrected his calculations on gravity in the light of this new value; it was not until June 1682 that he realized the significance of the French astronomer's work. When he did

so, he went through his calculations of sixteen years previously, making Picard's correction; and to his infinite joy found perfect agreement between theory and fact. It is said that, as the calculation proceeded, he became so excited that he had to get a friend to finish his calculation for him. But even then he hesitated as to announcing his conclusion that the force which draws the apple to the ground is the same force which controls the Moon in its orbit.

Meanwhile, Edmund Halley—a brilliant astronomer just rising into fame, fourteen years younger than Newton—had been investigating the problem of gravitation, and had proved independently of Newton, and eighteen years later, that the Sun's gravitation must vary as the square of the distance. As to finding the path under such an attraction, Halley was 'at sea', and he applied to Hooke and to Wren, better known as an architect, for light on the problem. Hooke thereupon stated that he had demonstrated 'all the laws of the celestial motion' by means of the inverse square law. Wren announced that if either Hooke or Halley could supply the necessary mathematical proof, he would present one or the other with a book of the value of forty shillings Hooke declared that he had the proof but 'would conceal it for some time, that others trying and failing might know how to value it when he should make it public'. Halley grew tired of waiting and went to Cambridge to see Newton. The first question which he put to Newton was, 'What would be the path of a planet under a gravitational attraction varying inversely as the square of the distance?' 'An ellipse,' Newton replied. 'How do you know?' asked Halley. 'I have calculated it,' Newton answered. The upshot of this was that Halley made Newton promise to transmit this discovery to the Royal Society. His paper, *De Motu*, was received by the Royal Society in February 1685.

Newton, now definitely immersed in the study of gravity, decided to prepare a large treatise on the subject, and in April 1685 he commenced to write the *Principia*, and a year

later Halley informed the Royal Society that 'Mr. Isaac Newton has an incomparable treatise on motion almost ready for the press'. On the 28th of April 1686 a manuscript entitled *Philosophiae Naturalis Principia Mathematica* was submitted by Newton to the Society. The importance of this was speedily perceived, and Halley was required to report to the Council as to the possibility of having the book printed, and on 19 May the Society resolved 'that Mr. Newton's work should be printed forthwith in quarto'. The Society, however, was short of money, and Newton himself was a poor man. What was to be done? On 2 June the Society passed a resolution that 'Mr. Halley undertake the business of looking after it and printing it at his own charge'. Halley agreed to do this, and defrayed the cost of publication. It has been claimed with justice that 'if Halley had done nothing more than secure the publication of the *Principia*, he would have been assured of the everlasting gratitude of posterity'.

The *Principia* was published in July 1687, and its author at once took rank as the greatest man of science of his day. It was a mighty task which Newton undertook. Just how mighty it was may be gathered from the words of one of his biographers:

'In the first place, the principles of dynamics had to be thoroughly grasped and clearly formulated, namely, that the absence of force or the balancing of the forces acting on a body means uniform speed in constant direction, without any acceleration, while any change in speed or in direction of motion of a body must be accounted for by a force or resultant of forces, proportional to the acceleration and acting in the same direction. Secondly, the type of force appropriate to the problem of planetary motion had to be considered and the inverse square law deduced. Thirdly, the physical reality of this attraction had to be proved, as presented by the motion of the Moon round the Earth, the gravitational effects of which we experience daily and can measure accurately. Fourthly, the law of gravitation had to be applied in its general form to the planets and the motions as given by Kepler's laws accounted for accurately. For this purpose new and more

powerful mathematical methods were required, and Newton had to invent them. Finally, the whole Solar System had to be considered under the aspect of universal gravitation, and motions of satellites and comets explained, precession and tides brought within the ambit of scientific research.'

Newton's popular fame rests of course upon his formulation of the law of universal gravitation:

'Every particle of matter in the Universe attracts every other particle with a force varying inversely as the square of their mutual distances and directly as the mass of the attracting particle.'

This bold and sweeping generalization is, of course, the pivot of the Newtonian cosmology. It explained at one and the same time the laws of falling bodies discovered by Galileo and the laws of planetary motion formulated by Kepler; Galilean and Keplerian laws were shown to be the inevitable outcome of Newtonian law. Under the sweep of the law of gravitation Newton effected a synthesis of the observed facts of astronomy and dynamics, so that a large number of apparently disconnected facts were shown to be the outcome of universal law; the arbitrary distinction between 'heaven' and 'earth', virtually done away with by Copernicus and his successors, was now finally obliterated.

Newton successfully explained, too, not only the motions of the planets, but some of their irregularities, and also such a baffling phenomenon as the precession of the equinoxes, which was shown to be due to the flattening of the Earth at the poles and the influence of Sun and Moon on a body which is not exactly spherical. The ebb and flow of the tides, too, received from Newton its first satisfactory explanation.

'And from the diurnal motion and the attractions of the Sun and Moon our sea ought twice to rise and twice to fall every day, as well lunar as solar, and the greatest height of the water to happen before the sixth hour of either day and after the twelfth hour preceding. By the slowness of the diurnal motion the flood is retracted to the twelfth hour; and by the force of the motion of

reciprocation it is protracted and deferred till a time nearer to the sixth hour. But till that time is more certainly determined by the phenomena, choosing the middle between those extremes, why may we not conjecture the greatest height of the water to happen at the third hour? For thus the water will rise all that time in which the force of the luminaries to raise it is greater, and will fall all that time in which their force is less; viz., from the ninth to the third hour, when that force is greater, and from the third to the ninth, when it is less.'

Further, Newton tackled comets, and succeeded in showing that these erratic bodies, regarded by the majority of people as supernatural visitants or atmospheric exhalations, were celestial bodies moving under the influence of gravity in very long ellipses. But he was unable to sketch any given orbit or to predict the return of a comet to the Earth's neighbourhood. This was reserved for his friend Halley early in the next century.

Just before the *Principia* appeared, Newton for the first time took part in public life, or rather was forced to do so. When King James II was engaged in endeavouring to restore Roman Catholicism in England, he came into conflict with the Universities. He attempted to force the University of Cambridge to confer a degree on a monk who had not taken the Oath of Allegiance. On the Vice-Chancellor's refusal, he and eight others were summoned to appear before the Court of High Commission in London. Of these eight, Newton was one. The Court admonished the representatives and deposed the Vice-Chancellor. After the Revolution Newton sat in the Convention Parliament as member for Cambridge University. A strong supporter of the House of Orange and a zealous Whig, he exercised no small influence in keeping Cambridge loyal to the new royal house. Newton was not, however, cut out for public life. He retired from Parliament in 1690, and although he served a later term as member for the same constituency, the sphere

of politics was not congenial. He was now in middle life, a confirmed bachelor, and his eccentricities were growing upon him; and with them came a certain irritability which distressed his friends. Further, he began to feel like a man with a grievance; he found himself left out in the cold when honours and appointments were being bestowed freely. However, in 1696 he was appointed Warden of the Mint, from which office he was promoted in 1699 to be Master of the Mint. Newton took his duties very seriously, and he carried through a complete recoinage. His appointment to the Mint necessitated his removal to London, and the capital city of England was from this time his permanent home. In 1699 he resigned his Cambridge chair.

Newton's epoch-making work in science was done before he was fifty—though he later busied himself with chemistry, history, exegesis, chronology, metaphysics, and theology. In the latter departments of thought his influence was far-reaching. For the mechanical view of the Solar System, so beautifully set forth by Newton, had no small influence in bringing about the rise of that particular brand of religious thought known as Deism.

Newton received in 1705 the honour of knighthood. His latter years were marred by some unfortunate controversies, chief among them that with Leibniz in connexion with the invention of fluxions. His official duties, however, took up most of his time during the last thirty years of his life.

By 1726, at eighty-three years of age, Newton's health was evidently failing rapidly. Nevertheless, he presided over a Royal Society meeting on 28 February 1727. On his return to his home in Kensington he became ill. He sank gradually, and passed away on 20 March 1727, in his eighty-fifth year. He was buried in Westminster Abbey, amid the mourning of a whole nation. He left behind him the memory not only of a man of superlative genius, but of real nobility of character. 'The whitest soul I ever knew,' was Bishop Burnet's

verdict on him. With true greatness he combined a real humility. 'If', he said, 'I have seen farther than most men, it is because I have stood on the shoulders of the giants'; and in a famous saying, with which we are all familiar, he compared himself to a little boy playing on the seashore, who had picked up one or two rare finds, while the great ocean of truth lay undiscovered before him.

III

AFTER NEWTON

JOHN FLAMSTEED—EDMUND HALLEY—JAMES BRADLEY—JAMES FERGUSON—JOSEPH LOUIS LAGRANGE—PIERRE SIMON LAPLACE.

THE dazzling brilliance of Newton's achievements has somewhat dimmed the fame of his two distinguished fellow countrymen and co-workers who filled in succession the office of Astronomer Royal. But the work of Flamsteed and Halley was of the highest quality, and they take rank among the greatest of English astronomers. To their indomitable perseverance in the face of obstacles which might have completely paralysed lesser men, as well as to their skill as observers, was due in no small measure the initial success of the great scientific institution over which they presided.

The first 'Observatory' to be founded, using that word in its true sense, was that connected with the University of Leyden in 1632; so just as Holland deserves credit for the first telescope, it deserves credit also for the first post-Galilean Observatory. The Paris Observatory was completed in 1671, and it was staffed under the supervision of Louis XIV himself, with a group of very able astronomers—Picard and Auzout, Frenchmen; Roemer, a Dane; and Cassini, an Italian who became director of the Observatory and held this post for many years. But despite the bright beginnings of the Paris Observatory, it was soon to be outstripped in efficiency by an institution four years its junior—the Royal Observatory at Greenwich. It was in England that observational astronomy was to make its chief advances, under three great observers—Flamsteed, Halley, and Bradley.

The foundation of the Observatory was due in the main to an accidental circumstance. It is a matter of common knowledge that there was in the post-Restoration period in England a wonderful quickening of interest in science, of which the

formation of the Royal Society was a manifestation. Sooner or later, an astronomical observatory was bound to have been erected, more especially as an institution of the kind had been set up in Paris in 1671. A Frenchman named Le Sieur de St. Pierre arrived in London about 1674 with the object of interesting English scientists in a scheme for the accurate measurement of longitudes. The scheme was remitted to a committee, of which John Flamsteed was appointed a member. Flamsteed was a young mathematician of great brilliance, whose work in astronomy had attracted a good deal of notice. He reported that the Frenchman's methods could not possibly be applied owing to the deplorable condition of observational astronomy. The positions of the stars, he pointed out, were not known with sufficient accuracy to allow its use.

The report of the committee was forwarded to the Government, and the sequel may best be described in Flamsteed's own words: 'I heard no more of the Frenchman after this, but was told that my letters had been shown King Charles. He was startled at the assertion of the fixed star place being false in the Catalogue, and said with some vehemence he must have them anew observed, examined and corrected for the use of his seamen.' We have little reason for looking back with pride on Charles II or his reign, yet we must admit that to the whim of this autocrat we owe the foundation of the Royal Observatory at so early a date as 1675. After the royal decree had gone forth, some controversy ensued as to the most suitable site. Chelsea and Hyde Park were both suggested, but on the recommendation of Sir Christopher Wren Greenwich Hill was chosen. A grant of £500 was made by the King. In addition, Charles provided bricks from Tilbury Fort, and iron, lead, and wood from an old gatehouse in the Tower which was in process of demolition. The foundation-stone was laid on 10 August 1675, and in a few years Flamsteed was in a position to begin his work in practical astronomy.

John Flamsteed was born at Denby, in Derbyshire, on

19 August 1646. He was a delicate and studious lad, whose feeble health had caused his education to be so sadly neglected that he was sixteen years of age before he began arithmetic. Having made a beginning, however, he became an apt pupil, and while still at school his interest was awakened in astronomy. With the aid of a hand-made quadrant he began to make simple astronomical observations, much to the annoyance of his father, who had destined him to be a merchant. But the delicate lad, though severely handicapped by the state of his health, which laid him aside for certain periods annually, was not to be diverted from his favourite study. Before he was twenty he constructed a catalogue of seventy fixed stars, and investigated the solar eclipse of 1666. In 1669 and 1670 he measured the movements of Jupiter and Mars among the stars, and even with his rough-and-ready instruments he ascertained the need for new planetary tables. Some work on occultations was sent by Flamsteed to the President of the Royal Society, and the outcome of his introduction to scientific circles was his decision to enter at Cambridge, where he took his degree in 1674, when he was twenty-eight years of age. He also took 'holy orders', so that throughout his career he was 'the Rev. John Flamsteed'. His original intention was to settle in a small parish near Derby, the 'living' of which was in the gift of his father. This, however, was not to be. The circumstances which brought about his appointment as the first Astronomer Royal of England have already been detailed.

By 1678 Flamsteed was fully immersed in the routine work of the new Observatory. Certainly this great institution did not have a very auspicious beginning. Despite King Charles' promise of further assistance, Flamsteed was heavily handicapped by lack of means. He was paid the beggarly sum of £100 a year, and he was absolutely single-handed; no assistant was provided to relieve him of the purely routine work of the Observatory. As he grew older, his health became

more indifferent and, it must be added, his temper more irascible. 'My distempers', he quaintly said, 'stick so close that I cannot remove them.' Nevertheless, for over forty years he toiled bravely on. His objective was a new star-catalogue to supplement that of Tycho Brahe, as the last pre-telescopic catalogue was by this time regarded as out of date. Flamsteed succeeded in fixing the positions of nearly 3,000 stars with greater accuracy than had ever been possible before. His star-atlas, long a standard work, was not published till ten years after his death, which took place on 31 December 1719, when he had reached the age of seventy-three.

The relations between Flamsteed and his great contemporary Newton were far from pleasant. Neither understood the other. Newton desired to use certain observations on the Moon which Flamsteed was making, and the Astronomer Royal seemed to take a delight in holding up these observations as long as possible. Later on the two quarrelled violently over the administration of the Observatory. Halley, Newton's friend, was Flamsteed's *bête noir*; and it was somewhat ironical that Flamsteed was succeeded as Astronomer Royal by the contemporary astronomer whom he most disliked.

Edmund Halley was born at Haggerston, Shoreditch, London, on 29 October 1656. His father, also called Edmund Halley, was a soap-boiler who had accumulated considerable wealth, and was able to give his son a good education. Young Halley, who early developed an aptitude for mathematics, received his early training at St. Paul's School, in London. By the time he had left school he had become proficient not only in mathematics but in astronomy as well. He set up an observatory of a kind in his father's house in Winchester Street, London, and made some observations of a solar eclipse.

At the age of seventeen he entered Queen's College, Oxford. His reputation went up to Oxford before him. 'Halley', said a contemporary, 'came to Oxford with skill

in Latin, Greek, and Hebrew, and such a knowledge of geometry as to make a complete dial.' At Oxford his taste for science was further developed, and after leaving, at the age of twenty, he decided to engage in practical astronomical work. At first he purposed taking up the work of fundamental astronomy, the determination of the exact positions of the stars. But on finding that Flamsteed, his senior by ten years, had just commenced work of this kind at the new Royal Observatory at Greenwich, he altered his plans. Since the northern skies were being systematically attended to, Halley conceived the idea of making an expedition across the equator so that he might observe the hitherto unexplored southern heavens; and he met with the most sympathetic consideration from his father, who provided him with an allowance of £300 a year in order to enable him to carry on his scientific work. In 1676, at the age of twenty, the young astronomer sailed in one of the East India Company's ships and after a quiet voyage landed at the island of St. Helena, which he had chosen for the site of his temporary observatory. Here he erected a telescope 24 feet long and a sextant of 5½ feet radius. He found the climate somewhat disappointing, with much rain and cloud, and his residence on the island extended over a year only. Notwithstanding this, he accomplished a great deal of pioneering work. His catalogue of the places of 341 southern stars, published in 1678, was the outcome of his observations; this catalogue is notable in astronomical history as the first drawn up with the aid of a telescope.

Halley's dash to the south seas had something of the romantic about it, and on his return he was hailed as the 'southern Tycho'. A Fellowship of the recently founded Royal Society was awarded him, while Oxford conferred on him the degree of Master of Arts without examination. Although only twenty-two years of age, he had become one of the most noted astronomers of his day. In 1679 he visited

Hevelius at Danzig, and in 1680 he went to Paris, where in conjunction with Cassini he made observations of the great comet of that year, a comet which riveted Halley's attention closely and gave him a permanent interest in that branch of astronomy with which his name was to be mainly associated. It is pleasant to note, too, that, despite the wars and rumours of wars which characterized the seventeenth century, there was so much of the international spirit among men of science.

In 1682 Halley married and settled down in a home of his own at Islington, where he set up a small private observatory. From this time onwards he was deeply immersed in the series of researches which led to the establishment of the law of gravitation. Reference has been made to the important part which he played, firstly in urging his friend Newton to announce his epoch-making calculations, and secondly, in making possible the publication of the *Principia.* It is no exaggeration to say, as has been said, that 'but for Halley, the *Principia* would not have existed'. Halley was in affluent circumstances, while Newton was poor; but many a man similarly placed, working at the same problem, and finding it solved by a rival worker, would not have gone out of his way to assist that rival to publicity and fame. But Halley seems to have been utterly disinterested and without a trace of self-seeking or jealousy. All he cared for was the discovery of truth and the advance of science.

After the *Principia* was published Halley devoted a great deal of his attention to working out the paths of comets, and he wrote a great deal of the material dealing with comets in the later editions of that immortal work. In 1705 he published a book entitled *A Synopsis of Cometary Astronomy*, in which he calculated the orbits of twenty-four comets. Newton had shown that comets obeyed the law of gravitation just as planets did, but he had not been able to map out any cometary path. This Halley, as the result of much labour, succeeded in doing. He had made a special study of the bright comet of

1682, and on going back over the older records he was impressed by the fact that bright comets very similar to that of 1682 had been in the habit of appearing at intervals of seventy-five or seventy-six years.

'There are', he said, 'many things which make me believe that the comet which Apian observed in the year 1531 was the same with that which Kepler and Longomontanus more accurately described in the year 1607; and which I myself have seen return, and observed in the year 1682. All the elements agree, and nothing seems to contradict this my opinion, besides the inequality of the periodic revolutions. Which inequality is not so great neither, as that it may not be owing to physical causes. For the motion of Saturn is so disturbed by the rest of the planets, especially Jupiter, that the periodic time of that planet is uncertain for some whole days together. How much more therefore will a comet be subject to such like errors, which rises almost four times higher than Saturn, and whose velocity, though increased but a very little, would be sufficient to change its orbit, from an elliptical to a parabolical one. And I am the more confirmed in my opinion of its being the same; for that in the year 1456, in the summer time, a comet was seen passing retrograde between the Earth and the Sun, much after the same manner; which though nobody made observations upon it, yet from its period and the manner of its transit, I cannot think different from those I have just now mentioned. And since looking over the histories of comets I find, at an equal interval of time, a comet to have been seen about Easter in the year 1305, which is another double period of 151 years before the former. Hence I think I may venture to foretell, that it will return again in the year 1758.'

In announcing to the Royal Society his conclusions about this comet, he used these words: 'If it should return according to our predictions, about the year 1758, impartial posterity will not refuse to admit that this was first discovered by an Englishman.'

As the year 1758 drew near, great excitement prevailed among men of science to see whether Halley's prediction

would be fulfilled. The French mathematician Clairaut, and two other mathematicians, undertook the task of calculating the exact date of the comet's return. The outcome of these researches was to show that the attraction of Saturn would delay the return of the comet by 100 days and that of Jupiter by 518 days. Men of science all over the world watched anxiously, and at last on Christmas Day 1758 the comet was sighted by an amateur, a farmer in Saxony. The comet reached its perihelion, or point nearest to the Sun, on 12 March 1759, and then disappeared on its long journey.

In 1835 Halley's comet again reappeared, and on 15 November 1835 passed the point of its path closest to the Sun. Three able mathematical astronomers undertook to calculate the exact date of the planet's perihelion passage. Damoiseau, a Frenchman, fixed on 4 November 1835; Pontécoulant, another Frenchman, fixed on 12 November; Rosenberger, a German calculator, taking account of the attractions of all the principal planets, fixed on 11 November. The perihelion passage actually took place on 15 November—a proof of the remarkable accuracy of the three calculators. In 1835 the comet was first detected at Rome, and was particularly studied by Sir John Herschel, who, on 5 May 1836, caught the last glimpse of it with his giant telescope. From 1836 to 1873 the comet was on its journey outward to the most remote point of its orbit, beyond the pathway of Neptune. In 1873 it reached its aphelion, as this farthest point is called, and then commenced returning with increasing velocity to the regions of light and heat. In November 1908 plates were exposed in the region of the heavens where it was calculated that the comet would appear, but it was not until September 1909 that it was actually discovered photographically by Dr. Max Wolf of Heidelberg. The comet in May 1910 was disappointing as a spectacular object to observers in Europe, owing to its unfavourable position for observation. Halley's is famous as the first

comet which was proved to revolve round the Sun in an elliptic orbit, and to be subject to the law of gravitation just as the planets are. Also, of all the known periodic comets, it is the one which has the longest period of revolution.

In 1716 Halley contributed to the Royal Society a paper on the 'Parallax of the Sun by the transit of Venus', in which he pointed out the advantages of making the most careful observations of this rare phenomenon. Two transits were due to take place, in 1761 and 1769, which Halley could not possibly hope to see, but he emphasized the extreme importance of these transits and urged astronomers to make adequate preparation for fitting out expeditions to carry through observations in distant lands. Observations of these transits did result in more accurate measures of the solar parallax than had ever been made before, though by no means so accurate as Halley had hoped for.

In 1718 Halley communicated another paper to the Royal Society which turned out to be of even greater significance. This paper dealt with the 'proper motions' of the stars. He found that Aldebaran, Sirius, Arcturus, and Betelgeux had unmistakably altered their places since Ptolemy's time by quantities which, though minute, were evidently real; in the case of Sirius, he noted a discrepancy between his own observations and those of Tycho Brahe. 'What shall we say then?' asked Halley. 'These three stars being the most conspicuous in heaven, are in all probability the nearest to the Earth; and if they have any motion of their own, it is most likely to be perceived in them.' This was the first definite proof that the stars were really in motion. Halley's other contributions to astronomy included a study of the brighter star-clusters; he was the first to draw attention to what he said was but 'a little patch', namely, the great cluster in Hercules. An eclipse of the Sun which took place in 1715 and was total in London was carefully studied by Halley, who drew attention to the corona and also to 'a very narrow

streak of a dusky but strong red light'—quite evidently the chromosphere.

In 1691 Halley had become a candidate for the new chair of Astronomy at Oxford. His candidature, however, was opposed by Flamsteed, with whom he was on consistently bad terms. As Halley got on well with other scientists, as well as with Newton, it would seem that if at this late date we try to assess the blame, the balance is on Flamsteed's side. Twelve years later, on the chair becoming again vacant, Halley was appointed, and he retained the post for seventeen years. Then in 1720, at an age when most men are thinking of retiring, Halley was appointed Astronomer Royal in succession to Flamsteed, who had died on the last day of 1719.

On taking up his duties at Greenwich an unpleasant surprise awaited him. Flamsteed's widow had removed all the instruments, and the Observatory stood derelict. No observations could be made. Halley entered into negotiation with Mrs. Flamsteed, and offered to buy the instruments, but so bitter did she feel that she flatly refused to part with any of them, although they were of no use to her. Halley was in a difficulty. However, in 1721 he obtained a grant of £500 from the Board of Ordnance, and a transit instrument and a quadrant were set up. Soon after, he commenced a series of observations on the Moon with a view to determining more accurately the irregularities in its motion, and thus to improve the lunar theory. This series of observations was planned to last over eighteen years, and it required no small degree of optimism for Halley to commence them at the age of sixty-four. He succeeded, however, in carrying out his programme. His health continued practically unimpaired till about 1738, when at the age of eighty-two he had a stroke of paralysis. He died at Greenwich on 14 January 1742, in his eighty-sixth year.

On Halley's death he was succeeded by James Bradley,

who was to maintain the high traditions of Greenwich Observatory.

The third Astronomer Royal was, indeed, in some respects the greatest man of the three, though he was more of a specialist than Halley and lacked that great man's capacity for moving from one department of the science to another.

James Bradley was born in 1693 at Sherborne in Gloucestershire, and received his education at the Grammar School of Northleach. On finishing his schooling he proceeded in 1711 to Balliol College, Oxford, where he graduated B.A. in 1714 and M.A. in 1717. His attention was first directed to astronomy by his uncle, James Pound, rector of Wansted in Essex. Pound was a man of great ability, and was reckoned as one of the ablest observers of his day. By means of observations of Mars in opposition he attempted in 1717 to measure the Sun's parallax. Of this attempt Halley wrote: 'Dr. Pound and his nephew, Mr. Bradley, did attempt, myself being present, in the last opposition of the Sun and Mars this way to demonstrate the extreme minuteness of the Sun's parallax, and that it was not more than twelve seconds nor less than nine seconds.' That is to say, this amateur and his young nephew fresh from Oxford showed that the Sun's distance probably lay between 93 and 125 million miles. Their lower limit was, of course, very near to the mark. Bradley's extreme skill as an observer led to early recognition in scientific circles, and in 1718 he was elected a Fellow of the Royal Society. In 1719 he took orders in the Church of England and became vicar of Bridstow, in Monmouthshire. Two years later he succeeded Halley in the chair of Astronomy at Oxford; and in 1742 he followed the same great man as Astronomer Royal. He held this high office for twenty years until his death on 13 July 1762, in his seventieth year.

Bradley's greatest discoveries, by which he is known to fame, were the aberration of light and the nutation of the

Earth's axis. The former discovery was made while he was attempting, vainly as it turned out, to measure stellar parallax. In 1725 he found that the star Gamma Draconis went through some annual motion which he first mistakenly thought might be due to parallax. After much puzzling on the strange variation, Bradley hit on the correct explanation.

'At last I conjectured', he said, 'that all the phenomena hitherto mentioned proceeded from the progressive motion of light and the Earth's annual motion in its orbit. For I perceived that if light was propagated in time, the apparent place of a fixed object would not be the same when the eye is at rest, as when it is moving in any other direction than that of the line passing through the eye and object; and that when the eye is moving in different directions the apparent place of the object would be different.'

Bradley had been searching for a measure of stellar parallax which would prove beyond doubt that the Copernican theory was true, and that the Earth was moving. He failed in this, but provided a proof of the Earth's motion as convincing as a parallax measure would have been.

His second discovery, that of the nutation of the Earth's axis, was a direct consequence of the first. He found that in the course of a year, when a star had completed the movement due to aberration, it did not return to the exact position which it had occupied. And he rightly concluded that this minute discrepancy was due to a slight change in the point of observation—the Earth itself. This is the 'nutation', or wobbling, of the Earth's axis. That Bradley was able to detect these minute irregularities with the comparatively primitive instruments at his command is testimony to his skill as an observer. But his most valuable work consisted in his observations of star-positions carried out in the last twelve years of his life. Sixty thousand observations were made in these twelve years and were published in 1798 and 1805, long after his death, in two large volumes. In 1818 an equally great observer, Bessel of Konigsberg, issued a catalogue

of 3,000 stars, based on Bradley's observations. These standard star-places for 1760 have been of the utmost value in subsequent research. In those investigations of proper motion which have issued in the modern discovery of star-streaming Bradley's observations are fundamental; they form the starting-point of modern statistical stellar astronomy, and have grown more precious, if anything, with the passage of years.

Before considering the work of the famous French mathematicians who carried Newton's work to its triumphant completion, reference should be made to those who, by disseminating and popularizing the Newtonian theory in universities and colleges and among the cultured section of the public, rendered much service to science. Curiously enough, this was done chiefly by Scotsmen. Before the end of the seventeenth century David Gregory was teaching the Newtonian system in Edinburgh University at a time when English academic circles were somewhat lukewarm or hesitant.

But perhaps the most prominent of all those who disseminated Newtonian principles was one who, though not in the front rank of astronomers, just missed attaining a place therein, and deserves an honourable place in any historical account of the progress of astronomy. This was the Scottish 'shepherd-boy astronomer', **James Ferguson.** He was born at Core of Mayen, near Rothiemay, in Banffshire, on 25 April 1710. His father, John Ferguson, was a poor farm labourer, and James was the second son. The future astronomer had little education. He learned to read unaided, and his father, a man of considerable intelligence, taught him to write. 'About three months I afterwards had at the Grammar School at Keith', wrote Ferguson, 'was all the education I ever received.'

At the age of ten the boy was sent by his father to keep sheep for a neighbour. While so employed he began to

study the stars. When he was fourteen years of age, in his own words, he

'went to serve a considerable farmer in the neighbourhood, whose name was James Glashan. I found him very kind and indulgent; but he soon observed that, when my work was over, I went into a field with a blanket about me, lay down on my back, and stretched a thread with small beads upon it at arm's length between my eye and the stars, sliding the beads upon it till they hid such and such stars from my eye in order to take their apparent distance from one another, and then, laying the thread upon a paper, I marked the stars thereon by the beads according to their respective positions, having a candle by me. My master at first laughed at me, but when I explained my meaning to him, he encouraged me to go on; and that I might make fair copies in the daytime of what I had done in the night, he often worked for me himself.'

Through Glashan Ferguson became acquainted with well-to-do people about Banffshire, and in time made his way to Edinburgh, and from thence to London, which was destined to be his home. For nearly seventeen years he earned a precarious livelihood by teaching and lecturing; while his mechanical genius, too, found outlet in the construction of numerous orreries, planetariums, astronomical clocks, and sundials. In 1748 he commenced his popular lectures on astronomy, which were then something of a novelty. But his chief title to fame is that of a writer on astronomy. In 1754 he completed his book, *An Idea of the Material Universe from a Survey of the Solar System*, and at this time he was engaged in the preparation of a greater work, *Astronomy explained upon Sir Isaac Newton's Principles*, which was published in London in June 1756. During the author's lifetime it went through six editions. Ferguson was now held in universal respect, and his work superseded for a great number of years all other books on astronomy. But fame did not bring wealth in its train, and for some time he was in very straitened circumstances.

Then in 1760, when his fortunes were at their nadir, King George III granted to Ferguson an allowance of £50 a year. Small though this amount seems to us of to-day, the pension saved Ferguson from financial ruin and for the remainder of his life he was in a fairly comfortable position. In 1763 a high honour was conferred on him: he was elected a Fellow of the Royal Society.

In 1761 Ferguson observed the transit of Venus from the top of the British Museum, using a 6-foot reflector. He remarked, 'I carefully examined the Sun's disc to discover a satellite of Venus, but saw none.' For some time before the transit he had been taking much interest in it, as it afforded the best means of measuring the Sun's distance. Two years later he sent a paper on his observations to be read before the Royal Society. He also observed the spots on the Sun, and left drawings of them, while in 1769 he published a description of the transit of Venus of that year, the last of the pair of transits visible during his lifetime. Ferguson died in London on 16 November 1776.

Ferguson was a man of remarkable sagacity, and he had more than one happy guess or clever intuition, though he lacked the power of steady application to any one line of study. In a paper written in 1756, at a time when the general view was that the Solar System was created ready-made, Ferguson threw out a remarkable hint as to the building up of the system by gravitational action. 'In the beginning', he wrote, 'God brought all the particles of matter into being in those parts of open space where the Sun and planets were to be formed, and endowed each particle with an attractive power, by which these neighbouring and at first detached particles would in time come together in their respective parts of space, and would form the different bodies of the Solar System.'

It was in France that the main work of eighteenth-century astronomy was done by a group of mathematicians of whom the chief were Euler, Clairaut, D'Alembert, Lagrange, and

Laplace. The main aim of this school was to show that the Newtonian theory was capable of explaining the observed motions of the bodies in the Solar System. The problem before these mathematicians was: given the eighteen known bodies in the Solar System—Sun, planets, and satellites—and their positions and motions at any given time, to deduce from their influence on one another in accordance with Newton's law their positions and motions at some later period, and to prove that these are in agreement with actual observation. The problem was a formidable one, and its complete solution is not yet, but the French mathematicians attacked it bit by bit, solving here a particular case and there a particular case, until by the end of the eighteenth century the last of the outstanding anomalies had been removed.

The greatest of these five mathematicians were Lagrange and Laplace, and their life-work must be briefly touched upon. **Joseph Louis Lagrange** was born at Turin on 25 January 1736. Although of Italian birth he was of a French family, as his name indicates, and as his main life-work was done in France he may be reckoned a Frenchman. Educated at Turin, he became when a mere boy professor at the Artillery School there, where most of his pupils were his seniors. In 1764 he won a prize offered by the Paris Academy on the libration of the Moon. In 1766 he accepted an invitation from Frederick the Great to become head of the mathematical department of the Berlin Academy. In a characteristically egoistic message of invitation, Frederick said that the greatest king in Europe wished to have the greatest mathematician in Europe at his court. Lagrange remained in Berlin for twenty-one years. In 1787, on the eve of the French Revolution, he obeyed a summons from Louis XVI to join the French Academy. In the following year there was published in Paris his greatest work, the *Mécanique Analytique*, which has been called 'one of the most beautiful of all mathematical books'.

Lagrange's chief attention was given to the secular alterations in the elements of a planet's orbit, a theme which has considerable bearing on the stability of the Solar System and its possible duration. In 1774 Lagrange in an elaborate essay considered the 'long inequality' of Jupiter and Saturn, which had aroused doubts in the minds of mathematicians as to the permanence of the system. Lagrange proved, however, that in the case of two planets perturbing each other, the variation of their nodes and orbital planes would oscillate within certain limits, and would not result in radical changes. In 1776 he further showed that the perturbations between two planets could never result in any continuous variation in their distances from the Sun—no steady increase or decrease of distance. In 1782, in a veritable *tour de force*, he showed that this conclusion was valid also in the case of all the planets perturbing one another. So far as Lagrange could see, the Solar System was in a stable state—no likelihood of disruption or catastrophe so far as the human mind could calculate. Lagrange died on 10 April 1813.

Pierre Simon Laplace was born at Beaumont-en-Auge, near Honfleur, on 22 March 1749. He was the son of a small farmer, and thanks to the assistance of some kindly neighbours he was able to carry on his education at the Military School of his native town. At the age of eighteen he became a teacher in the Military School in Paris. This was in 1767, and for the next sixty years Laplace lived in Paris, where he held various official positions, and devoted himself continuously to the study of theoretical astronomy. Like his colleague, Lagrange, he passed unscathed through the turmoil of the French Revolution, but he bought his immunity from imprisonment and from death by a course of political procedure strongly reminiscent of the Vicar of Bray. When the Revolution came he was a revolutionary; when Napoleon emerged over the political horizon he became a Bonapartist. Indeed, he was appointed Minister of the Interior when

Napoleon was First Consul, but was obliged to retire on the grounds of incompetence. He was, however, designated as a member of the Senate. When the Empire was proclaimed Napoleon made Laplace a Count, but on the restoration of the Bourbons he swung over to the Royalist side and was created a Marquis. These changes of opinion were not very creditable to Laplace, and indicate that despite the greatness of his mental qualities there was an unscrupulous strain in his nature which made him a sycophant and a time-server. Laplace died on 5 March 1827, in his seventy-eighth year, nearly a century after the passing of Newton.

Laplace left behind him two books which will ever rank as standard volumes in the library of astronomy. The chief was the *Mécanique Céleste*, which appeared in five volumes between 1799 and 1825. This book was a kind of compendium of all that had been achieved in theoretical astronomy since Newton's time. Of this book, the eminent historian, Miss Clerke, said:

'The work is a record of unmixed triumphs. With grave exultation Laplace proceeds from point to point, recounting the events of the campaign, commemorating the battles won by the brilliant staff of mathematical heroes to which he himself belonged. . . . He scarcely looked beyond. There was indeed no "beyond" where his methods of investigation were applicable. The *Mécanique Céleste* hints at no unsatisfied ambitions.'

The scope of the work may be gauged from his own résumé of it in his third volume.

'We have given, in the first part of this work, the general principles of the equilibrium and motion of bodies. The application of these principles to the motions of the heavenly bodies has conducted us, by geometrical reasoning, without any hypothesis, to the law of universal attraction; the action of gravity, and the motions of projectiles on the surface of the earth, being particular cases of this law. We have then taken into consideration a system of bodies subjected to this great law of nature; and have obtained,

by a singular analysis, the general expressions of their motions, of their figures, and of the oscillations of the fluids which cover them. From these expressions we have deduced all the known phenomena of the flow and ebb of the tide; the variations of the degrees, and of the force of gravity at the surface of the earth; the precession of the equinoxes; the libration of the Moon; and the figure and rotation of Saturn's rings. We have also pointed out the cause why these rings remain, permanently, in the plane of the equator of Saturn. Moreover, we have deduced, from the same theory of gravity, the principal equations of the motions of the planets; particularly those of Jupiter and Saturn, whose great inequalities have a period of above nine hundred years.

"The inequalities in the motions of Jupiter and Saturn presented, at first, to astronomers nothing but anomalies, whose laws and causes were unknown, and, for a long time, these irregularities appeared to be inconsistent with the theory of gravity; but a more thorough examination has shown that they can be deduced from it; and now these motions are one of the most striking proofs of the truth of this theory.'

The Solar System was portrayed in the pages of Laplace to be a stupendous machine, moving under the influence of immutable law. And further, the System, Laplace concluded, had all the appearance of permanence. Amid many secular changes, said Laplace, 'we have discovered the constancy of the mean motions and of the mean distances of the bodies of this system; which nature seems to have arranged, at its origin, for an eternal duration, upon the same principles as those which prevail so admirably upon the Earth, for the preservation of individuals and for the perpetuity of the species.' Looking down the vista of the future, then, Laplace saw no probability of the dissolution of the Solar System, no possibility of anything approaching a break-down of the stupendous celestial machine which he had essayed to portray.

In the passage above quoted Laplace referred casually to the 'origin' of the Solar System. It was in his second great masterpiece, the *Exposition du Système du Monde*, which has

been characterized as 'one of the most perfect and charmingly written popular treatises on astronomy ever published, in which the great mathematician never uses either an algebraical formula or a geometrical design', that he put forward 'with that distrust which everything ought to inspire that is not the result of observation or calculation', his famous nebular hypothesis of the origin of the Solar System. 'We are astonished', he said, 'to see all the planets move round the Sun from west to east, and nearly in the same plane, all the satellites moving round their respective planets in the same direction and nearly in the same plane with the planets.' The Sun and planets, too, he pointed out, rotate on their axes in the same direction, from west to east. 'A phenomenon so extraordinary is not the effect of chance; it indicates a universal cause, which has determined all these motions.'

Thus Laplace reached his conclusion by reasoning backward from the remarkable coincidences in the planetary system. The cause of these, he concluded, 'must have been a fluid of immense extent'. To have given in the same direction a nearly circular motion round the Sun, the fluid must have been a kind of solar atmosphere, which originally extended far beyond the limits of the present Solar System. As the 'atmosphere' or nebula contracted, the planets were formed by 'the condensation of zones', while the satellites were formed in a similar way. The five coincidences in the planetary motions, and in the inclination of the planetary orbits, which Laplace pointed out, naturally follow from this hypothesis, 'to which the rings of Saturn add an additional degree of probability'.

Various obstacles to the unqualified acceptance of the Laplacian theory were obvious even before the death of its author. On the original theory no body in the Solar System could revolve in a retrograde direction: yet, during Laplace's lifetime, Herschel had discovered the retrograde motions of the

Uranian satellites. A hundred years later three other instances of retrograde satellites were discovered—the ninth satellite of Saturn at Harvard in 1898, and the two outermost moons of Jupiter at Greenwich and the Lick Observatory in 1908 and 1914. In 1861 it had been proved by Babinet, a French mathematician, that the axial motion of the hypothetical solar nebula extending to the orbit of Neptune would have been so slow that a single revolution would have required 27,000 centuries. Under such conditions the centrifugal force could never have counterbalanced the attractive force, and as a consequence there could have been no detachment of rings or separation of planetary bodies from the parent mass. Proctor, too, pointed out in 1874 that 'Laplace's great nebulous contracting mass is a very unsatisfactory conception to begin with. Laplace's theory does not in any way correspond with processes taking place within the Solar System. It gives no account of the immense number of meteor flights and comets still existing within the solar domain.' And the late Sir G. H. Darwin showed that a ring of matter distributed uniformly would in all likelihood collapse on the mass from which it was detached. According to the Laplacian theory, too, satellites must revolve round their primaries more slowly than the latter rotate on their axes, for the central body in contracting rotates more and more swiftly. But the inner of the two satellites of Mars, discovered in 1877, revolves three times for one rotation of its primary. Similar remarks apply to the inner ring of Saturn, the meteoric components of which revolve in about half the time required by the planet to rotate on its axis.

Any one of these difficulties might perhaps in itself be overcome, but their cumulative effect is fatal, not to the nebular hypothesis as such, but to Laplace's particular form of the theory. But Laplace's brilliant hypothesis will ever remain one of the epoch-making theories in the history of

science. Faulty in detail, the nebular hypothesis is valid in its main presupposition—that the Solar System has developed, in the course of the centuries, from the simple to the complex, from the diffuse dust-cloud of the immeasurably distant past to the Sun and planets as we know them to-day.

IV

THE HERSCHELS

FRIEDRICH WILHELM HERSCHEL—CAROLINE LUCRETIA HERSCHEL—JOHN FREDERICK WILLIAM HERSCHEL

BY common consent one of the greatest astronomers who have ever lived, **Friedrich Wilhelm Herschel** was born at Hanover on 15 November 1738. He came of an old German family, and was descended from Hans Herschel, one of three brothers who had been driven out of Moravia early in the seventeenth century on account of steadfast devotion to the Protestant religion and had settled in Saxony. Hans Herschel spent the greater part of his life in the Saxon town of Pirna. His son, Abraham, was trained as a landscape-gardener and was employed in this capacity first in Dresden and later at Hohentziatz, in the principality of Anhalt-Zerbst, near Magdeburg. According to the short account of the family given by his illustrious grandson, 'he had also a good knowledge of arithmetic, writing, drawing, and music'. The last-named talent he bequeathed to his youngest son Isaac, who at the age of twenty-one took up music as his life-work. Despite his abilities, however, he failed to make good. After holding appointments in Brunswick and Potsdam he made his way to Hanover, where he became hautboy-player in the Foot Guards. Hanover was destined to be his home, and a year after his settlement there he married a daughter of a citizen of the neighbouring town of Wenstadt. Six of their ten children—four sons and two daughters—reached maturity. Friedrich Wilhelm was the second son.

Isaac Herschel, despite the bad luck which dogged his footsteps throughout life, was a man of wide general culture, as well as musical genius. His wife, a plain, dull woman, had a great aversion to learning, holding that people ought to be content in the station into which they had been born.

But only the elder daughter inherited her mother's dullness. The remaining five were all distinguished people. Jacob, Alexander, and Dietrich were eminent musicians, while the younger daughter Caroline earned a fame in science only second to that of Friedrich Wilhelm himself.

In a memoir written in old age, Caroline Herschel penned some interesting recollections of her father. He was, she said,

'a great admirer of astronomy and had some knowledge of that science; for I remember his taking me out on a clear frosty night into the street to make me acquainted with several of the most beautiful constellations after we had been gazing at a comet which was then visible. And I well remember with what delight he used to assist my brother William in his various contrivances in the pursuit of his philosophical studies, among which was a neatly-turned 4-inch globe, upon which the equator and ecliptic were engraved by my brother.'

The post of a bandsman in the Hanoverian Guard was not a lucrative one, and Isaac Herschel was forced to augment his small salary by giving private lessons in music. All through life his circumstances remained straitened, and his poverty was aggravated by a chronic asthmatical affection contracted as the result of lying in a wet furrow after the battle of Dettingen in 1743. Having no worldly goods to bequeath to his children, he sought to provide them with the best education possible under the circumstances; and from their earliest days he instructed them in music. William Herschel recorded that his father

'taught me to play on the violin as soon as I was able to hold a small one made on purpose for me. . . . Being also desirous of giving all his children as good an education as his very limited circumstances would allow, I was at a proper time sent to a school where besides religious instruction all the boys received lessons in reading, writing, and arithmetic: and as I very readily learned every task assigned to me, I soon arrived at such a degree of perfection, especially in arithmetic, that the master of the school

made use of me to hear younger boys say their lessons and to examine their arithmetical calculations.'

At the age of fourteen and a half William Herschel entered the band of the Hanoverian Guard. Although his school life was at an end, his education was only beginning. For two years he received private lessons from a teacher named Hofschläger, who afterwards filled an important post at Hamburg. These lessons included languages, logic, ethics, and metaphysics. 'Although', Herschel wrote in after years, 'I loved music to excess and made considerable progress in it, I yet determined with a sort of enthusiasm to devote every moment I could spare to the pursuit of knowledge, which I regarded as the sovereign good, and in which I resolved to place all my future views of happiness in life.'

The family circle was, for the time being, broken up in 1755. The times were stormy: the Seven Years' War was raging: a French invasion of England was anticipated, and the Hanoverian Guard was drafted across the North Sea. Isaac Herschel and his two sons left Hanover with the regiment. Embarking at Cuxhaven in the end of March 1756, they reached Chatham after a passage of sixteen days.

After nine months the Guards were ordered back to Hanover, owing to the French threat to the country. Early in the following year the regiment went into the campaign which culminated in disaster at Hastenbeck on 26 July 1757. Young Herschel did not like this, his first and only experience of military life. Accordingly, in his own words, he 'left the engagement and took the road to Hanover, but when I arrived there I found that having no passport I was in danger of being pressed for a soldier.' At that time Herschel was not technically a soldier, but a member of the band. So he returned to the regiment, only to find that 'nobody had time to look after the musicians—they did not seem to be wanted.' The forced marches in the hot weather affected his health, and his father advised him to leave the service.

'In September my father's opinion was, that as on account of my youth I had not been sworn in when I was admitted to the Guards, I might leave the military service. Indeed, he had no doubt but that he could obtain my dismission, and this he after some time actually procured (in 1762) from General Spörcken, who succeeded General Sommerfeld.'

The formal discharge paper is in existence and was printed for the first time in the *Collected Scientific Papers of Sir William Herschel*, published in 1912. Dr. Dreyer, in his introductory sketch of Herschel's life, gave it as his view that 'the existence of this formal discharge paper puts an end to the legend, too long and too readily believed, that he deserted from the army and that he received a formal pardon for this offence from George III on the occasion of his first audience in 1782'. It is indeed difficult to determine whether Herschel was technically a deserter or not. In some notes furnished in later years to a Göttingen scientific periodical, Herschel said, 'In my fifteenth year I enlisted in military service, only remaining in the army, however, until my nineteenth year, when I resigned and went over to England.' On the other hand, as already noted, he gives it as his father's view that he was not really a soldier at all. The formal discharge paper is dated 29 March 1762, so that if William Herschel was ever actually a unit of the army, the discharge paper merely registered an accomplished fact: he had been out of the army and out of Germany for four and a half years.

At Hamburg Herschel was joined by his brother Jacob, and they embarked together for England, where they had for some time a hard struggle to make ends meet. Jacob gave up the struggle and returned to Hanover; William, after a time of great hardship, succeeded in procuring an appointment in Yorkshire. He made rapid progress in the musical profession and after holding appointments at Leeds and Halifax he was appointed in 1766 organist of the Octagon Chapel at Bath. This post he held for sixteen years and he combined with it

the profession of music teacher. Pupils flocked to him, and his lessons at times numbered thirty-five per week. He had now a settled home in England, and all he required was some one to look after it. At this stage the idea of marriage does not seem to have entered into his head, and he decided to send to Hanover for his young sister **Caroline Lucretia Herschel** (born 16 March 1750), who since her father's death had had a very uncongenial existence as household drudge under an unsympathetic and unlovable mother. She gladly accepted her brother's invitation, and arrived in Bath in the autumn of 1772. Thus was inaugurated a partnership which was to be dissolved only by death and which was to be of the greatest significance in the history of science.

For it was in astronomy, not in music, that Herschel was to make his mark. The stars had attracted him from boyhood; and after his settlement in England he made observations from time to time. At the time of his sister's arrival he was experiencing one of his periodic revivals of interest in the subject. He read with great interest Ferguson's *Astronomy explained upon Sir Isaac Newton's Principles*. This book kindled his interest into enthusiasm. So keen did he become that, in his own words, 'I resolved to take nothing upon trust but to see with my own eyes all that other men had seen before.' In May 1773 he procured some object-glasses which he fitted into pasteboard tubes, and with the best of these rudimentary telescopes he observed Jupiter. But he soon discovered the weakness of the refracting form of telescope in the unpleasant effect called chromatic aberration; and so he turned his attention to the reflector. He decided to acquire a mirror to be fitted into a tube 5 or 6 feet long, but he found there were none in the market of so large a size. 'A person', his sister recorded in her memoirs, 'offered to make one at a price much above what my brother thought proper to give.' This did not discourage him. He decided to make a mirror for himself, and having procured the appa-

ratus of a friend who had been amusing himself in trying to grind mirrors, he plunged into the work of telescope-making. By the spring of 1774 he had succeeded in making a telescope good enough to point to the heavens. On 1 March 1774 he made his first entry in his astronomical journal, stating that he had viewed 'the lucid spot in Orion's sword belt, and the ring of Saturn, which appeared like two slender arms'.

For the next eight years he carried on the two professions of musician and astronomer, cramming the work of two lives into one. He observed the Moon, Jupiter, and Mars. His work on the last-named planet, begun in 1777, was indeed epoch-making; for it was he who first directed attention to the white spots at the poles, known as the polar caps, and correctly surmised their true nature. With a 7-foot Newtonian reflector he began in 1776 his first 'review of the heavens'. This was a mere preliminary. Much more thorough was his second review, in the course of which he examined stars down to the eighth magnitude. He commenced this review in 1779, and in the course of it he made a discovery which caused the whole world to ring with his fame. On Tuesday, 13 March 1781, he jotted down in his journal the following note, in somewhat doubtful English: 'In the quartile near Zeta Tauri, the lowest of two is a curious either nebulous star or perhaps a comet. A small star follows the comet at two-thirds of the field's distance.' In the paper afterwards communicated to the Royal Society he explained that he perceived a star which appeared 'visibly larger than the rest: being struck with its uncommon magnitude, I compared it to H Geminorum and the small star in the quartile between Auriga and Gemini, and finding it so much larger than either of them, suspected it to be a comet.' On Saturday, 17 March, he wrote, 'I looked for the comet or nebulous star, and found that it is a comet, for it has changed its place.' By Monday, the 19th, he found that the supposed comet 'moves according to the order of the signs, and its orbit declines but little from the ecliptic'.

The discovery was communicated to the Observatories of Greenwich and Oxford. Maskelyne, the Astronomer Royal, stated on 4 April that he had observed the strange object, 'very different from any comet I ever read any description of or saw'. On 23 April he wrote to Herschel, 'It is as likely to be a regular planet moving in an orbit nearly circular round the Sun as a comet moving in a very eccentric ellipsis.' Attempts were made to calculate its orbit, on the assumption that it was a cometary body. These efforts were fruitless, and eventually Lexell, the St. Petersburg mathematician, announced that the mysterious object was not a comet at all, but an exterior planet, revolving at twice the distance of Saturn, thus confirming Maskelyne's sagacious surmise. Later it transpired that the planet had been observed no less than seventeen times between 1690 and 1781, by able astronomers such as Flamsteed, Bradley, and Mayer, all of whom failed to differentiate it from an ordinary star, either with respect to its appearance or its motion.

No small sensation was aroused by Herschel's achievement. It was the first planetary discovery within the memory of man —Mercury, Venus, Mars, Jupiter, and Saturn having been known from prehistoric times. More wonderful still, the discovery had been made, not by the leading astronomers of the day, but by an unknown amateur. At one bound Herschel leaped from obscurity to fame. The Royal Society of London awarded him the Copley medal in November 1781, and elected him a Fellow in December. The discovery had aroused interest in still 'higher' circles, and on 10 May 1782 it was intimated to Herschel that King George III expected to make his acquaintance; and on 28 May he had an audience of the King, to whom he presented a drawing of the Solar System. On 2 July Herschel noted in his diary— 'I had the honour of showing the King and Queen and the Royal Family the planets Jupiter and Saturn, and other objects.' Herschel suggested for the newly discovered body

the name of 'Georgium Sidus' in honour of his royal patron, but Continental astronomers rightly refused to accept the suggestion. Lalande named the new body 'Herschel', but Bode's name of 'Uranus', in keeping with the customary method of naming the planets, prevailed.

Herschel was now seriously considering the possibility of abandoning the profession of music and devoting himself to astronomy. After George III expressed interest in the discovery, Herschel indicated that he was anxious to be made 'independent of music'; and the result of his interview with the King was his appointment as King's Astronomer, referred to by Herschel himself in his journal in the following terms: 'It was settled by His Majesty that I should give up my musical profession and, settling somewhere in the neighbourhood of Windsor, devote my time to astronomy.'

In August 1782 William and Caroline Herschel entered into possession of a large house on Datchet Common, near Windsor. Here they remained till 1785, but the house was damp, and a sharp attack of ague convinced Herschel that he must, for his health's sake, find other quarters. In June he removed to Clay Hall, near Old Windsor, and in April 1786 to Slough, destined to become one of the 'shrines' of astronomical science, 'the spot of all the world', said Arago, 'where the greatest number of discoveries have been made'. Here for many years Herschel and his devoted sister worked from twilight to dawn, sweeping for clusters and nebulae, counting the stars in limited regions of the heavens, occasionally scrutinizing the Moon and the planets. 'If it had not been', writes Caroline, 'for the intervention of a cloudy or moonlit night I know not when he or I either would have got any sleep.' In the daytime, too, his activity was ceaseless. He had to attend to his telescopes and to direct the army of workmen who were constantly employed making repairs: in addition, he was actively employed systematizing his observations and co-ordinating his results, which

appeared in the long series of papers contributed to the *Philosophical Transactions* of the Royal Society.[1]

Caroline Herschel notes that it was her brother's chief object at this time to construct a 30-foot or 40-foot instrument. But nothing could be done without a government grant; Herschel could not afford the expense of constructing a great telescope for himself. After some preliminary spade-work had been done in the proper quarters by his lifelong friend, Sir William Watson, Herschel asked Sir Joseph Banks, President of the Royal Society, to make application for a grant from the King. In September 1785 a grant of £2,000 was made, and a start was made with the making of the instrument. Two years later a second sum of £2,000 was granted, and in addition Herschel received, over and above his salary, £200 per annum for the upkeep of the telescope; while a salary of £50 a year was bestowed on Caroline Herschel as her brother's assistant. By this time her own name was becoming famous in the scientific world. On 1 August 1786, during Herschel's absence in Germany, she discovered a comet, the first of eight of these bodies to her credit. The small annuity conferred upon her was a recognition—painfully inadequate—of her own work in astronomical science.

The construction of the telescope occupied nearly four years. Caroline recorded that

'there is not one screwbolt about the whole apparatus but what was fixed under the immediate eye of my brother. I have seen him lie stretched many an hour in a burning sun, across the top-beam, while the iron-work for the various motions was being fixed. At one time, no less than twenty-four men (twelve and twelve relieving each other) kept polishing day and night: my brother, of course, never leaving them all the while, taking his food without allowing himself time to sit down to table.'

This ceaseless industry bore fruit when in August 1789

[1] Published for the first time in 1912 as *The Collected Scientific Papers of Sir William Herschel.*

the great telescope was ready for use. Herschel's former telescopes had been Newtonians, with small secondary mirrors. In January 1787, however, he made a novel experiment with his 20-foot telescope. In order to save the light lost by the second reflection, Herschel removed the small mirror and slightly tilted the tube. The result more than justified expectations, and the experiment resulted in the detection of two faint satellites of Uranus. He decided therefore to make the 40-foot telescope on this 'front-view' principle.[1]

Herschel was justly proud of his large telescope, yet, on the whole, its performances were disappointing. Immediately it was finished Herschel succeeded in confirming the existence of two new satellites of Saturn; and he used his great instrument on Saturn and its rings on numerous occasions. But it was cumbersome and difficult to manipulate, and the speculum on which so much care had been bestowed preserved its original polish only for two years. In Herschel's later years he seldom used it, although it remained standing until seventeen years after its maker's death. In 1839 it was dismantled by Sir John Herschel, and laid in a horizontal position, which it occupied for many years, until all but 10 feet of the tube was destroyed by a falling tree.

The completion of the '40-foot' was the climax of Herschel's career as a maker of telescopes. The fame of the great instrument spread over the world. Princes, dukes, and courtiers did not fail to visit Slough to view one of the wonders of the age. Men of science, too, came from all parts of the world. Herschel did not abandon telescope-making altogether, but there was not now the same necessity from a pecuniary point of view. On 8 May 1788 Herschel was married to Mrs. Pitt, widow of Mr. John Pitt, and daughter of Mr. Adee Baldwin, a London merchant. Miss Burney, the novelist, has left on record her meeting with Herschel and his wife soon after the marriage. 'His newly-married wife was with him, and his

[1] This particular form of the reflector is known as the Herschelian.

sister. His wife seems good-natured: she was rich, too! And astronomers are as able as other men to discern that gold can glitter as well as stars.' Whether or not there is any ground for this hint as to a motive for Herschel's marriage, there can be no doubt that he was now relieved from all financial care. Herschel's later years were busy and happy. Although he cared little for such things, various honours were conferred on him, and in 1817 he became a knight of the Royal Hanoverian Guelphic Order.

A serious illness, ten years earlier, had left his health permanently impaired. Nevertheless he carried on bravely, and some of his best work was done when the infirmity of old age was upon him. He died on 25 August 1822, in his eighty-fourth year, and was interred in the Church of St. Lawrence at Upton. The Latin epitaph on his tombstone there claims with justice that *coelorum peruppit claustra*, 'he broke through the barriers of the skies'.

Prostrated with grief, his devoted sister decided to leave England and to make her home in her native city of Hanover. She had not long settled there when she realized her mistake. For twenty-five years she led what she called 'a solitary and useless life—not finding Hanover or any one in it like what I left when the best of brothers took me with him to England in August 1772'.

Solitary her life was, but by no means useless. Soon after her settlement in Hanover she formed a catalogue of all her brother's nebulae and clusters, arranged in zones. In April 1825 she forwarded this to her nephew, John Herschel, then engaged in his review of these objects. This catalogue, described by Sir David Brewster as 'an extraordinary monument of the unextinguished ardour of a lady of seventy-five in the cause of abstract science', was rewarded by the presentation to her of the Gold Medal of the Royal Astronomical Society in 1828—an honour by which, with characteristic modesty, she said she was 'more shocked than gratified'.

In 1835 she was elected an honorary member of the Royal Astronomical Society, membership of which was not then open to women. But such honours sat lightly upon her. 'Saying too much of what I have done', she said in 1826, 'is saying too little of him, for he did all. I was a mere tool which he had the trouble of sharpening and adapting for the purpose he wanted it, for lack of a better.' On 9 January 1848 she passed away—within two months of completing her ninety-eighth year—and was buried beside her parents in the churchyard of the Gartengemeinde at Hanover. Her epitaph, composed by herself, records that 'the eyes of her who is glorified were here below turned to the starry heavens. Her own discoveries of comets and her participation in the immortal labours of her brother, William Herschel, bear witness of this to future ages'.

We cannot do more than enumerate Herschel's investigations and discoveries. His work on the Sun was of a high order; he was perhaps the first to investigate systematically and exhaustively the form and motion of sun-spots, and it is rather remarkable that he failed to detect the existence of the solar cycle. His solar work has been somewhat undervalued because of his adherence to what was called the Wilsonian theory of the Sun's constitution. Alexander Wilson, Professor of Astronomy in Glasgow University, had in 1774 put forward the theory that the Sun was a dark solid globe, surrounded with a hot and luminous atmosphere, the sun-spots representing rents in that atmosphere through which the dark globe could be seen. 'The solid body of the sun beneath these clouds', Herschel said, 'appears to be nothing else than a very eminent, large and lucid planet, evidently the first, or in strictness of speaking, the only primary one of our system: all others being truly secondary to it.' He went indeed so far as to say that in all likelihood this super-planet was 'richly stored with inhabitants'. There can be little doubt that this theory considerably retarded the progress of solar astronomy.

Of the bodies in the Solar System only the Moon and Mercury can be said to have been neglected by Herschel; and he did observe the Moon from time to time. His observations on Venus lasted over sixteen years—1777 to 1793. His conclusions as to Venus were largely negative. He satisfied himself that the spots on its surface were faint and ill defined, and that the mountains announced by other astronomers did not really exist. His work on Mars was epoch-making. He rediscovered the polar caps: Maraldi had seen them in 1719, but Herschel was unaware of Maraldi's observations and made the discovery independently. 'I may well be permitted to surmise', he wrote in 1784, 'that the bright polar spots are owing to the vivid reflection of light from frozen regions: and that the reduction of light from these spots is to be ascribed to their being exposed to the Sun.' His general conclusion regarding Mars was that 'the analogy between Mars and the Earth is perhaps by far the greatest in the whole Solar System'. The planet, he concluded, 'has a considerable but moderate atmosphere, so that its inhabitants probably enjoy a situation in many respects similar to ours'.

His work on Jupiter led him to propound the 'trade-wind' theory of the belts; and he concluded that each of the four satellites rotated as the Moon does, with one hemisphere turned constantly towards Jupiter. He discovered two satellites of Saturn—the tiny inner moons known as Mimas and Enceladus, and he concluded that the outermost satellite rotated in a manner similar to the Moon. In 1789 he detected the division in the ring. In 1787 he discovered two satellites of his own planet Uranus. His supposed discovery of four others in 1797 was not confirmed either by himself or any other observer.

Herschel's stellar work was, of course, his main preoccupation. In the course of his long-continued study of the stars for their own sakes, he made two discoveries of first-class importance. The first of these, announced in 1783,

was the motion of the Sun, carrying with it the planets and their satellites. According to the Copernican system, the Sun is at rest relative to the Earth, and Copernicus did not stop to inquire whether the day-star might have a motion of its own. Kepler certainly believed the Sun to occupy the centre of the Universe; but, after Halley's discovery of the proper motion of four bright stars, astronomers began to think that the Sun might also be in motion. 'If the proper motion of the stars in general be once admitted,' asked Herschel, 'who can refuse to allow that our Sun, with its planets and comets, is no less liable to such a general agitation as we find to obtain among all the rest of the celestial bodies?'

Herschel accordingly resolved, right at the beginning of his career as King's Astronomer, to search for evidence of such a proper motion. It was evident to him that if the Sun were moving, its motion could only be detected by a general drift of the stars in a contrary direction. If the Sun is moving in a certain direction, the stars in front will appear to disperse, while those behind will seem to draw nearer together. If the stars were all at rest, the problem would be easy of solution. But the stars themselves have their own individual motions, and this complicates matters considerably. Herschel saw that the minute proper motion of each star would have to be decomposed into two components, the star's real motion and an apparent motion, the reflection of the Sun's movement in the opposite direction. Very few proper motions were known with accuracy at the end of the eighteenth century. Dealing with seven bright stars—Sirius, Castor, Procyon, Pollux, Regulus, and Altair—he separated the two components in each case by simple geometrical methods and concluded that the Sun was moving towards a point in the constellation Hercules near to the star Lambda Herculis. 'We may', he said, 'in a general way estimate that the solar motion can certainly not be less than that which the Earth has in her annual orbit.' In 1805 Herschel again attacked the problem

with more data to work upon, and his result was in the main confirmatory of his earlier result. The astronomers of the next generation felt somewhat dubious. Even Herschel's son believed the data on which his father worked to have been too slender. In 1837, however, Argelander, after a most elaborate and critical discussion, abundantly confirmed the conclusion of Herschel, and each subsequent investigation has been confirmatory. Herschel thus proved then that just as the Earth is a moving planet like the other planets, so the Sun is like its fellows, a moving star.

The second outstanding discovery made by Herschel was announced in 1802. Double stars had long fascinated him. He was not, it is true, the first discoverer of double stars. The first telescopic observers noted the first of these strange twin stars, as they could not help doing. But they attracted no particular attention till Herschel began his surveys of the heavens. In 1782 he communicated to the Royal Society a catalogue of 269 doubles, of which he had himself discovered 227: and in 1784 he drew up a second list of 484. From the very beginning of his work on doubles he seems to have suspected that many of them must be real and not merely visual doubles, that there could not possibly be so many cases of two stars happening to lie nearly in the same line of vision and seeming in consequence to be double. But it was not until 1802 that Herschel found evidence of orbital motion in a number of pairs. 'Casual situations', he announced on 1 July of that year, 'will not account for the multiplied phenomena of double stars.' Many double stars, he said, 'have actually changed their situation with regard to each other, in a progressive course, denoting a periodical revolution round each other'. In 1804 he brought forward conclusive evidence that many doubles are 'not merely double in appearance, but must be allowed to be real binary combinations of two stars intimately held together by the bond of mutual attraction'. The significance of this discovery may be realized when we

recollect that previously there was no scientific proof that the law of gravitation prevailed outside of the Solar System. There were, of course, the strongest theoretical reasons for believing that it did prevail, but it was not until Herschel's long-sustained study was brought to a conclusion that men were assured of the universal validity of the Newtonian law and of the unity of the Cosmos.

'A knowledge of the construction of the heavens', Herschel wrote in 1811, 'has always been the ultimate object of my observations.' To attain to this knowledge was the master-passion of his life. The chief object of his surveys of the heavens was to gauge the extent of the Stellar System by counting the number of the stars visible in different regions. In 1785 he put forward his disc-theory of the Stellar System, according to which the Milky Way was an optical effect due to the shape of the system. He sketched it as a thin cloven disc of irregular outline, the cleft representing the well-known division in the Milky Way. In this system, which he believed to extend much farther in the galactic plane than in the direction of the poles, he believed the Sun to be placed near but not quite at the centre—75 'astronomical units' from the north galactic pole and 80 from the south, and 597 units from the boundary of the system in the direction of Aquila, and 352 from the boundary in the direction of Canis Major. An astronomical unit Herschel took to be the computed mean distance of stars of the first magnitude. He believed the Galactic System to be strictly limited in extent, and to be, in fact, only one system among others—an 'island universe'. 'Our nebula', he maintained, 'is a very extensive branching compound congeries of many millions of stars.'

The name 'our nebula' was significant. So great had been his success in resolving into separate stars the milky spots catalogued by Messier in 1783, that he confidently believed all nebulae to be clusters of stars which would yet be resolved

by higher telescopic powers. All these nebulae he considered to be 'island universes'. In 1785 he announced, indeed, that he had discovered 'fifteen hundred whole sidereal systems, some of which might well outvie our Milky Way in grandeur'. He divided these nebulae, or universes, into four classes or 'forms'—which differed in the degree of condensation or clustering. 'We inhabit', he said, 'the planet of a star belonging to a compound nebula of the third form.'

The original disc-theory, with its corollary, the hypothesis of island universes, was expounded for many years in text-books of astronomy and manuals of popular science as if it had been held by Herschel throughout his life in the exact form in which it was promulgated. There can be no doubt that, as Struve and Proctor maintained after their careful study of Herschel's series of papers, the great astronomer altered his opinions. Indeed, it is surprising that for half a century there was widespread ignorance of this change of view, for William Herschel himself frankly avowed it. He wrote in his paper of 1811: 'I must freely confess that by continuing my sweeps of the heavens, my opinion of the arrangement of the stars and their magnitudes and of some other particulars has undergone a gradual change.' He was led to a modification of his views along two lines of research. In his first papers he had assumed an approximately equal scattering of stars, though he was quite clearly of opinion that the assumption was an approximation only. In his paper of 1785 he admitted that 'in all probability there may not be two or three of them in the heavens, whose mutual distance shall be equal to that of any other two given stars, but it should be considered that when we take all the stars collectively there will be a mean distance which may be assumed as the general one'. Even in the same paper Herschel remarked that it would not be difficult to point to two or three hundred 'gathering clusters' in the Stellar System. Accordingly he began to foresee as a result of what he called 'the clustering

power' the breaking up of our Stellar System into many small independent nebulae. From this it was but a step to his recognition in 1802 of the fact that 'this immense starry aggregation is by no means uniform. The stars of which it is composed are very unequally scattered, and show evident marks of clustering together into many separate allotments.' Herschel was thus driven to abandon his general view of the Stellar System as a collection of myriads of 'insulated stars', and to substitute for this the conception of a system consisting of many local groups and clusters.

His persistent study of the nebulae resulted in a change in his view of these objects and in the abandonment of the 'island universe' theory as a universal generalization. He was led by gradual stages to question his earlier theory that nebulae were simply distant clusters; but it was in his paper on 'Nebulous stars properly so-called' that he announced his belief in the existence surrounding such stars of a 'shining fluid of a nature totally unknown to us'. From this he came to reject the stellar theory of nebulae, first in the case of planetary nebulae, then in that of the diffused nebulosities, such as that in Orion. The new view of nebulae as gaseous masses led to a revised estimate of their distances. They came to be regarded as part of the general Galactic System, and the theory of island universes fell into the background.

Did Herschel then abandon the disc-theory? If by the disc-theory we mean the detailed hypothesis of 1785 which viewed the system as composed of evenly distributed 'insulated' stars, the answer is in the affirmative; for in the course of his investigations Herschel came to recognize not only the existence of binary stars, but of local groups and aggregations, especially in the galactic plane. But if by the disc-theory we mean the general outline of the Stellar System with much greater extension along the plane than in the direction of the poles, then Herschel most certainly did not abandon it. In 1817 and 1818 he communicated two papers

to the Royal Society on the extent of the Milky Way, and on the relative distances of star-clusters. In these papers he outlined his new method of star-gauging, which has been confused by numerous writers with the first. The two methods, however, were essentially distinct. In the first, one telescope was used on different areas of the sky: in the second, on the other hand, the same region was examined by different instruments. Once again Herschel assumed for the sake of investigation a certain 'properly modified equality of scattering', and a certain equality of real brightness. In the paper of 1817 he applied this method to the Milky Way, and in that of 1818 to clusters, assuming that the relative distances of globular and other clusters can be determined by the telescopic powers necessary to reveal and resolve them, and also that the component stars are, generally speaking, comparable to Sirius in size. His ultimate conclusion was that the Stellar System was considerably more extended in the plane of the Galaxy than he had previously believed. 'The utmost stretch of the space-penetrating power of the 20-foot telescope', he wrote in 1818, 'could not fathom the profundity of the Milky Way: and the stars beyond its reach must have been farther from us than the 900th order of distance.' In 1818 his final conclusion was that when our gauges will no longer resolve the Milky Way into stars, it is 'not because its nature is ambiguous'—possibly nebulous—'but because it is fathomless'; and he still held to the view that some of the dim misty objects visible with his telescope were island universes.

As has been mentioned, Herschel's observations of some of the nebulae led him to believe in the existence of 'a shining fluid of a nature totally unknown to us'. He first assumed the existence of this shining fluid in 1791, and he suggested that it was 'more fit to produce a star by its condensation than to depend on the star for its existence'. This was five years before Laplace propounded his nebular hypothesis. The germ of the nebular theory, therefore, was present in

the mind of Herschel even at this early stage. In the paper of 1791 Herschel proceeded to apply his new view to the various nebulous regions all over the heavens. He concluded that he had been too hasty in his former surmise that all nebulae were distant clusters. If the 'shining fluid' can exist without stars, 'we may with great facility explain that very extensive telescopic nebulosity' in the constellation Orion. 'What a field of novelty is here opened to our conceptions!'

In 1802 Herschel dealt with the subject again in his catalogue of 500 new nebulae. But it was not till 1811, in another epoch-making paper on the construction of the heavens, that Herschel enunciated his nebular hypothesis. In this paper he gave a complete list of nebulae, which he had discovered and studied, 'assorting them into as many classes as will be required to produce the most gradual affinity between the individuals contained in any one class with those contained in that which precedes and that which follows it'. He declared it highly probable that 'every succeeding state of the nebulous matter is the result of the action of gravitation upon it while in a foregoing one, and by such steps the successive condensation of it has been brought up to the planetary condition. From this, the transit to the stellar form, it has been shown, requires but a very small additional compression of the nebulous matter.' Herschel's nebular hypothesis has never received in text-books of astronomy the attention it deserves. It was the result of long years of patient study, and is one of the most perfect examples of inductive reasoning in the history of science.

When old age began to creep over Herschel and his physical powers were no longer equal to the tremendous tasks which he had set himself, he had the intense satisfaction of seeing his own son decide definitely to follow in his footsteps and to carry on his work.

John Frederick William Herschel was born at Slough near Windsor on 7 March 1792. He was a somewhat delicate

child and, being much in the company of his father and his aunt, mature beyond his years. As one of his most intimate friends said of him:

> 'His home was singular, and singularly calculated to nurture into greatness any child born as John Herschel was, with natural gifts capable of wide development. At the head of the house there was the aged, observant, reticent philosopher and, rarely far away, his devoted sister Caroline Herschel. . . . It was in the companionship of these remarkable persons, and under the shadow of his father's wonderful telescope, that John Herschel passed his boyish years. He saw them in silent but ceaseless industry, busied about things which had no apparent concern with the world outside the walls of that well-known house.'

His aunt left record of his boyish pranks and experiments. 'John and I', she wrote long afterwards, 'were the most affectionate friends, and many a half or whole holiday spent with me was dedicated to making experiments in chemistry, in which generally all boxes, tops of tea-canisters, pepper-boxes, teacups, &c. served for the necessary vessels, and the sand-tub furnished the matter to be analysed.' After a short period at a preparatory school at Hitcham he was enrolled at Eton. His parents, however, hearing that he was being maltreated and persecuted by a school bully, withdrew him from Eton after a few months. He was not again sent to school. His father engaged as private tutor a Scotsman named Rogers, an able mathematician, who coached him so well that he was able to enter St. John's College, Cambridge, at the early age of seventeen. His university career was one sustained triumph. His aunt records that from the time of his matriculation to graduation, he gained, without exception, all the first prizes for which he was eligible. Despite the competition of some exceptionally brilliant men, he graduated as Senior Wrangler and Smith's Prizeman at the age of twenty-one. A Fellowship of Trinity College was followed by the degree of Master of Arts, conferred in 1816.

While still a student Herschel communicated a paper to the Royal Society, and that distinguished body elected him a Fellow at the unprecedentedly early age of twenty-one. A series of memoirs on mathematical subjects gained for him the Society's Copley Medal before he was thirty. Apparently a scientific career does not seem to have been his father's ideal for him, nor his own for himself. His father desired him to enter the clerical profession; doubtless he felt that, like other distinguished men, his son might follow out his scientific interests and at the same time work towards his youthful ambition 'to leave the world better than he found it'. But the young man himself, though from his earliest years of a religious and even devout turn of mind, did not favour the project, and chose to read for the law. He entered as a student at Lincoln's Inn in January 1814, but he felt no clearer call to the bar than to the pulpit, and after a short period of legal study he decided to abandon the idea of a professional career and devote himself to science. Then, as now, this was a hazardous proceeding for a man without means. But Herschel was fortunate—much more so than his father had been at his age. William Herschel had been in very comfortable circumstances since his marriage, and his son was in a position to follow his bent.

His main scientific interests at this stage were pure mathematics and optics. Despite, indeed, perhaps because of, the heavily charged astronomical atmosphere of his home, he felt as a young man no special interest in the stars. On 10 September 1816, however, he informed a correspondent that he was 'going to take up star-gazing' under his father's direction. Herschel senior was naturally anxious that his clever son should follow in his footsteps and round off his own career. It has been said that 'it was through filial reverence that he resolved to tread in his father's footsteps'. And even at this early stage of his career he had a strong tendency to discursiveness, a tendency which characterized him throughout his

life. 'I find it impossible', he told one of his friends, 'to dwell for very long on one subject, and this renders my pursuit of any branch of science necessarily very desultory.' So doubtless he felt his astronomical apprenticeship rather irksome at first. However, it was not long until astronomy became his dominant interest: so that, in spite of his contributions to other branches of science and learning, it is as an astronomer that he is remembered.

His first specialized piece of work was on double stars. His father had virtually founded what—to coin a clumsy term—may be called 'double-star astronomy'. A few doubles had been accidentally discovered before William Herschel's time, but it was that great astronomer who, besides discovering many doubles, systematically measured for the first time the relative position of the components of close pairs. These measures led to the discovery in 1802 that some double stars at least are binaries, both stars revolving round their common centre of gravity. Doubtless William Herschel advised his son as to the precise line he should follow in astronomy; at all events he began to observe and measure his father's double stars at Slough in 1816. These observations were continued at Slough until Sir William's last illness. After his father's death he worked in conjunction with a friend, James South, at a private observatory belonging to the latter in Southwark. South appears to have been as interested in double stars as Herschel, and together they measured 380 of Sir William's doubles.

Sir William Herschel died on 25 August 1822. He had been in a very feeble state throughout the year, but the end came unexpectedly. Indeed, so little prepared was his son for his death that he had set off for Holland about a fortnight before, and was not, therefore, in those days of slow travel, able to be present at his father's funeral. From now onwards he resided at Slough with his widowed mother, until her death in 1832. These were the years of Herschel's greatest

activity. With his 20-foot telescope, made by himself under his father's supervision in 1820, he carried through a vast amount of work. This instrument for a time was the last word in telescopes. Referring to the nebulae in Virgo, he informed his aunt in 1815: 'These curious objects I shall now take into my especial charge—nobody else can see them.' With this fine instrument he discovered no fewer than 3,347 double stars, and he succeeded in rediscovering the two satellites of Uranus which his father had detected but which had since been lost. His chief work, however, was on nebulae, and as his biographer, Miss Clerke, wrote, his sweeps of the heavens appear 'truly wonderful when we remember that he was without a skilled assistant'. In this respect he was much less happy than his father, for the work which Caroline carried through as secretary and general assistant was work of the highest order. In John's case 'no ready pen was at hand to record what he saw, and how he saw it: he was by necessity his own amanuensis; and writing by lamplight unfits the eye for receiving delicate impressions.' Nevertheless his 'sweeps' of the heavens resulted in a catalogue of 2,307 nebulae, of which 525 were new discoveries, presented to the Royal Society in 1833. At this stage of his career Herschel held to his father's hypothesis of the diffuse nebulae such as that in Orion, namely, that they are composed of 'a shining fluid, of a nature totally unknown to us', and that they represented the primeval world-stuff from which the suns and worlds of the future would be evolved.

Herschel's great catalogue attracted much attention—a good deal more attention than his father's had done. He received a knighthood in 1831, at the age of thirty-nine, and medals were bestowed on him by the Royal Society and the Royal Astronomical Society. By the time these were actually awarded, Herschel had embarked on the biggest enterprise of his life, the exploration of that part of the sky visible only in the southern hemisphere. He had early conceived the idea

of completing his father's work by gauging and sweeping in the southern heavens, but solicitude for his aged mother forbade him realizing his ideal during her lifetime. After her death in January 1832, at the age of eighty-one, his plans for this great enterprise went forward rapidly.

The southern skies were in those days much neglected by astronomers. There were one or two observatories in the southern hemisphere—at Parramatta in Australia and at the Cape of Good Hope. But these were in their infancy, and besides were devoted to specialized work. There had been, it is true, expeditions to the southern hemisphere. Halley's in 1676 was the first; in the eighteenth century Lacaille, a famous French astronomer, repeated Halley's performance. But although these astronomers had made some important observations, the southern skies had never been explored systematically with a large telescope. To John Herschel we owe the first detailed study of this region of the heavens.

At first Herschel inclined to an Australian location for his temporary observatory. But after mature consideration he decided in favour of South Africa, and on 13 November 1833 he sailed from England, accompanied by his wife and family. On his arrival two months later he lost no time in selecting a site for his instruments. He chose Feldhausen, 'a perfect paradise in rich and magnificent mountain scenery', four or five miles from Cape Town. Here he erected his 20-foot reflector in February 1834, and his refractor, mounted equatorially in a revolving dome, four months later. By October 1835 he informed his aunt, 'I have now very nearly gone over the whole southern heavens and over much of it often.' His surveys resulted in the discovery of 1,202 double stars, and 1,708 clusters and nebulae. He directed special attention to the great systems called the Nubeculae or the Magellanic Clouds. He was the first to make detailed analyses of these objects and he found their constitution 'to be of astonishing complexity', consisting of single stars, clusters, and nebulae.

He remarked too on the fact that these clouds are situated in a barren region of the sky. 'The access to the Nubecula Minor on all sides', he wrote, 'is through a desert.' Another object which fascinated him was the great Argo nebula. He drew up a catalogue of 1,203 stars projected on the nebula, and devoted several months to an attempt to delineate it. These were pre-photographic days, and it was no easy task to make a drawing of an object so complex as this, one of the greatest of the diffuse nebulae. The nebula filled him with wonder and admiration. 'Language cannot easily convey', he wrote, 'a full impression of the beauty and sublimity of the spectacle this nebula offers when viewed in a sweep ushered in by so glorious and innumerable a procession of stars, to which it forms a sort of climax.' He was fortunate, too, in witnessing the remarkable outburst of the star Eta Argus embedded in the nebula. On 16 December 1837 he found that it had increased threefold in brightness and it continued to increase till it became equal to Alpha Centauri, after which it gradually faded. When Herschel viewed it for the last time in March 1838, it was equal to Aldebaran. He believed, mistakenly in all probability, that Eta Argus was unconnected with the nebula, which in his view was merely its background.

But the most important work which he carried through in South Africa was his programme of star-gauging. He gauged 2,300 star-fields and counted 70,000 stars. These southern star-gauges told the same story as the northern: 'Nothing can be more striking', wrote Herschel, 'than the gradual but rapid increase of density on either side of the Milky Way as we approach its course,' which confirmed his belief that 'the plane of the Galaxy is to sidereal what the ecliptic is to planetary astronomy, a plane of ultimate reference, the ground-plan of the Sidereal System.' From an analysis of his gauges, however, he concluded that 'it would appear that with an almost exactly similar law of apparent

density in the two hemispheres, the southern were somewhat richer in stars than the northern, which may, and not improbably does, arise from our situation not being precisely in the middle of its thickness, but somewhat nearer to its northern surface.'

Practically every observable celestial body was passed in review by Herschel during those crowded years. As if his night work of sweeping the heavens was not sufficient to exhaust his energies, he attacked the problem of the Sun and its spots, being fortunate enough to observe in March 1837 one of the greatest spot-groups ever recorded, with an area of 3,780 millions of square miles. While adhering at this stage of his career to his father's erroneous theory of the Sun, he advanced a cyclonic theory of the origin of sun-spots which has been justly characterized as 'a decided advance in solar physics'. He was fortunate too in being at the Cape on the occasion of the nineteenth-century return of Halley's comet, which was visible from October 1835 to May 1836. His study of it was exhaustive; he noted remarkable changes in its head and tail—extensive variations in the apparent diameter of the head and periodic disappearances of the tail. His drawings of the comet rank among pre-photographic classics.

In March 1838 Herschel and his family sailed for England. His residence in South Africa had lasted for four years, and in a letter to his brother-in-law he described these years as 'the sunny spot in my whole life, where my memory will always love to bask'. On his return, laden with his scientific spoils, he was acclaimed as a conquering hero. Honours were showered upon him—a baronetcy, honorary degrees, banquets, and receptions. Much of this adulation was distasteful to him, for he was a man of shy and retiring nature and humble withal. Henceforth he occupied a place all his own in public esteem. Indeed, he bulked much more largely than his father—a much greater man—had ever done.

When Herschel returned from the Cape he was only forty-six, yet his observing career was at an end. He did not remount his great instruments. It is said, indeed, that he never again looked through a telescope. Miss Clerke, his biographer, rightly stresses the contrast between the father and son. 'He was then forty-six, two years younger than his father when he began his course of prodigious activity at Slough. Sir William's craving to see and know was insatiable: Sir John's was appeased by the accomplishment of one great enterprise. . . . One cannot but regret that in the plenitude of his powers and instructed by rare experience, he should have put by his weapons of discovery.' As already remarked, Herschel was a man of many interests; and in addition, despite his retiring disposition, public duties were thrust upon him as they had never been on his father. Among the offices which he held was that of Master of the Mint, which was a full-time appointment that left him little leisure.

But though his observing career was over, his astronomical career was not. He had brought back with him from South Africa a vast amount of observational material, and the time devoted to science was fully mortgaged in reducing and systematizing these and in publishing his results and conclusions. From 1840, when he removed from Slough to Collingwood in Kent, until 1847 he was engaged in the preparation of his monumental work, *Results of Astronomical Observations at the Cape of Good Hope*. Two years later he published his *Outlines of Astronomy*, an amplification of an earlier work. The book had a great vogue. It ran through twelve editions and was translated, not only into practically every European language, but also into Chinese and Arabic. For years it was the standard text-book. His later astronomical work was on the nebulae and clusters—a kind of summation of the results of his own observations and those of his father. After many years of spade-work, his *General Catalogue of Nebulae*, containing 5,079 nebulae and star-clusters, appeared

in 1864 in the *Philosophical Transactions* of the Royal Society. This great catalogue, revised and enlarged by Dr. Dreyer in 1888, remains a standard work. This was Herschel's last great effort. He planned a catalogue of double stars on a similar scale, but the infirmity of old age frustrated his purpose.

At an early stage of his life, before his South African expedition, he conceived the idea of collecting and editing his father's papers scattered through the *Philosophical Transactions*. This project he put by till a more convenient season which never arrived. It is to be regretted that such was the case. It would seem that he never read through these papers carefully, for he was apparently unaware of any progressive development in his father's views on the construction of the heavens. In his *Treatise on Astronomy* he referred briefly to his father's disc-theory of 1785 without any indication that it had been modified. In the *Outlines* also the disc-theory was reproduced and the greater richness of the southern skies explained on the basis of that theory. But, as a matter of fact, Herschel does not seem to have held any clear-cut hypothesis of the Stellar System. In the same edition of the *Outlines* in which he set forth the disc-theory as probably true, he considered favourably another theory altogether, namely that the Galaxy is a ring of stars surrounding the main Galactic System. On this theory the fainter galactic stars were faint, not because of excessive distance, but owing to intrinsic faintness. John Herschel thought that in those regions where the Milky Way is clearly resolved into stars 'well separated and seen projected on a black ground', and where presumably 'we look out beyond them into space, the smallest visible stars appear as such, not by reason of excessive distance but of a real inferiority of size or brightness'. Concluding that in the remoter regions of the Galaxy there exist 'innumerable individuals equal in intrinsic light to those which immediately surround us', he deduced for such stars a distance of 2,000

light-years, which would give about 4,000 light-years for the diameter of the Stellar System. On the status of the star-clusters and the nebula he does not seem to have had any decided opinion. At one time he appears to have practically ceased to believe in his father's 'shining fluid', though he lived to see the nebular theory rehabilitated in 1864 by the spectroscopic observations of Huggins on the Orion nebula and some planetaries; and he seems to have inclined at the close of his life to the view that all clusters and nebulae were included in one Stellar System.

After some years of increasing weakness, Sir John Herschel died at Collingwood on 5 May 1871, in his eightieth year. He was laid to rest in Westminster Abbey, close to the grave of Newton. His wife, the daughter of a Scottish minister, survived him for several years; and he left behind him a family of seven daughters and three sons. An eighth daughter predeceased her father. Two of his sons became famous in the scientific world. The second, Alexander Stewart Herschel, who occupied a chair in Durham College, did some good work in meteoric astronomy; while the third, John Herschel, carried out some spectroscopic investigations on the southern nebulae.

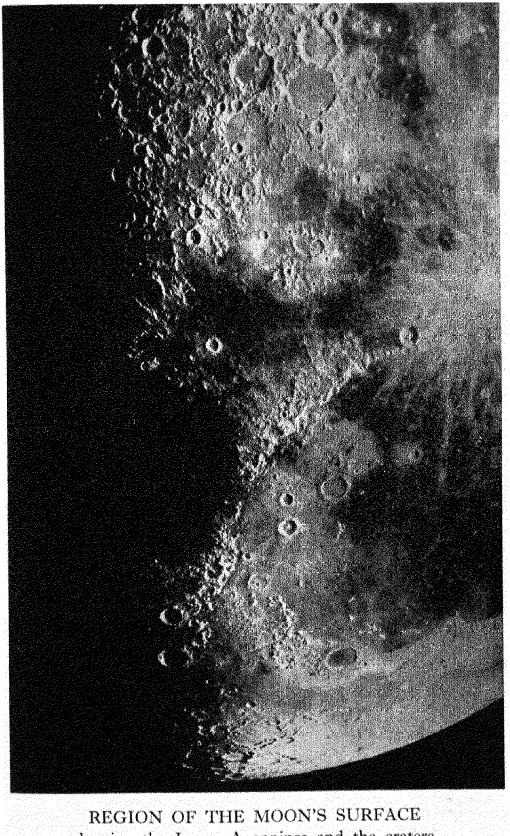
V

IN HERSCHEL'S FOOTSTEPS

JOHANN HIERONYMUS SCHRÖTER—HEINRICH WILHELM MATTHIAS OLBERS—FRIEDRICH WILHELM BESSEL—HEINRICH SAMUEL SCHWABE—JOHANN FRANZ ENCKE—FRIEDRICH GEORG WILHELM STRUVE—JOHANN HEINRICH MÄDLER—FRIEDRICH WILHELM AUGUST ARGELANDER—THOMAS HENDERSON—URBAN JEAN JOSEPH LE VERRIER—JOHN COUCH ADAMS.

TOWARDS the close of the eighteenth century there was in Germany a rapid growth of interest in astronomy. One of Herschel's biographers has remarked that there was at that time 'something in the air of Hanover and its neighbourhood that turned the eyes of young men of genius to the stars'. If for 'Hanover and its neighbourhood' we substitute the one word 'Germany', we are nearer the truth—although a large proportion of Germany's famous astronomers did come from the north-western part of the country. In the late eighteenth and early nineteenth centuries Germany was disunited, rent by civil strife, and harassed by foreign invasion; and yet in the midst of all this a school of astronomers flourished who advanced the science in all its departments. From one point of view Herschel was the first and greatest of this school. His interest in astronomy dated from his boyhood in Germany, and we may conclude with some degree of confidence that he would have become an astronomer even if he had stayed in Germany. Although long resident in England, his influence and example did not a little to stimulate the growth of the science in his native land.

It certainly was a potent influence in directing the thought of **Johann Hieronymus Schröter** to the stars. This distinguished astronomer was not quite seven years younger than Herschel. Born at Erfurt on 30 August 1745, he was sent by his father, when his elementary education was completed,



REGION OF THE MOON'S SURFACE
showing the Lunar Apennines and the craters
Plato and Archimedes

to the University of Göttingen, where he graduated in law, but where he studied also mathematics and physical science. And this taste for science was stimulated by the friendship which he formed with the Herschel family in Hanover. Schröter cannot have been at this stage acquainted with William Herschel, for the latter had left Hanover in 1757, when Schröter was a boy of twelve. Herschel's elder brother Jacob held an appointment in the court orchestra at Hanover: he and his two younger brothers were musicians of distinction and Schröter's interest in music brought him into contact with them. Doubtless his talks with the Herschels covered other subjects than music, and he was led to emulate the example of the brother in England who, in the midst of a busy career, was devoting his leisure to astronomy.

Schröter did not follow music as a profession, nor did he follow science. He did not diverge from his choice of law. In 1778, at the age of thirty-three, he was appointed Chief Magistrate of Lilienthal, near Bremen. His judicial and administrative duties were not too heavy, and left him with sufficient leisure to follow out the study of astronomy. A year after his settlement at Lilienthal he acquired a small refractor; but the refractor was then an unsatisfactory instrument. A few years later Herschel blossomed into fame as a maker of reflecting telescopes superior to anything ever before constructed. Many of them found their way to Germany, and one was acquired by Schröter in 1785. This was the chief instrument of his small observatory until 1792, when Schrader, an optician of Kiel, transferred his workshop to Lilienthal and constructed for Schröter a 13-foot reflector. This instrument, however, was considerably less satisfactory than the 7-foot, for Herschel was a master of his craft.

Schröter has been with justice called the founder of selenography, or the intensive study of the Moon's surface. His observations extended over a period of twenty-eight years. He detected large numbers of new craters and mountains, and

first drew attention to the curious formations known as 'rills' or clefts, of which he discovered eleven. He devised, too, a new method of measuring the altitudes of lunar mountains. More important, he laid a foundation on which those who came after him could build. True, he laboured under the handicap of bad draughtsmanship and he lacked that faculty of subordinating theory to observation which was so notable a characteristic of his greater contemporary and fellow countryman, Herschel, and it has been said that the value of his work was thus impaired. Like Herschel and the later eighteenth-century astronomers, he believed the Moon to be a living world, with volcanoes in active eruption and an appreciable atmosphere. And he was certainly too eager to register indications of this on somewhat slender evidence, as when he believed himself to have detected traces of a lunar atmosphere. Making all allowances for this preconception, we must acknowledge that selenography dates from Schröter.

In 1788 Schröter commenced observing Venus with the idea of ascertaining the period of rotation. Certain hazy, ill-defined markings were noted by him and from his observation of them he determined the period at 23 hours 21 minutes, in agreement with the value derived by Cassini in 1666. This conclusion was confirmed in 1811. He obtained definite evidence of an atmosphere enveloping the planet. He noted, too, that one of the horns or 'cusps' of the planet presented a blunted appearance, and he interpreted this as due to the existence of a high mountain twenty-three terrestrial miles in height. This somewhat extravagant conclusion brought him into controversy with Herschel, who bluntly said, 'As to the mountains on Venus, I may venture to say that no eye which is not considerably better than mine, or assisted by much better instruments, will ever get a sight of them.' This criticism of his work greatly vexed Schröter, who stated that he 'could not reconcile it to the friendly sentiments which the author has always hitherto expressed towards me and

which I hold extremely precious'. In 1800 Schröter's study of Mercury led him to conclude that the little planet rotated in 24 hours 4 minutes, and his later observations, reduced by his assistant Bessel after his death, seemed to confirm this estimate. His observations led him to believe the surface of Mercury to be rugged and mountainous, and as in the case of Venus, he regarded the blunting of one of the horns of the crescent to be due to the existence of a lofty mountain. His work on Mars was much less satisfactory, and he was wide of the truth in his conclusion that the Martian surface-markings represented nothing more than a shell of drifting cloud.

Schröter's observing career came to an end in 1813. He was indirectly a war casualty. In 1810 the Napoleonic troops occupied the electorate of Hanover. Schröter was dismissed from his post of chief magistrate and reduced to penury. He still carried on gallantly, however, taking refuge from the troubles of the Earth in the study of the heavens. But on 20 April 1813 the French troops, under General Vandamme, committed a wanton outrage. They occupied Lilienthal and proceeded to give it to the flames. Three days later they looted the observatory and burned it to the ground, and many of Schröter's precious observations were destroyed. Schroter never got over this stroke of fate. He was a ruined man financially, and he was without the means to repair the damage. And as his country was ruined too by the French invasion, no one could help him. He died on 29 August 1816, the day before his seventy-first birthday.

Heinrich Wilhelm Matthias Olbers was born at Arbergen, a village near Bremen, on 11 October 1758. His father, a Lutheran minister, was an enthusiastic student of science, and when at thirteen years of age young Heinrich took up astronomy he received every encouragement from his father. It is recorded that his love for astronomy was awakened by an evening walk in August 1771, when his boyish interest

was aroused by the Pleiades rising above the eastern horizon 'like a swarm of fire-flies tangled in a silver braid'. He read all the books on astronomy which he could lay hands on, and with the aid of star-charts he made himself familiar with the constellations.

But there was no intention on his part to embark on an astronomical career. In those days such a career was a blind alley, so when in 1777 he enrolled as a student at Göttingen, it was as a student of medicine. Nevertheless he did not neglect physical science, and studied mathematics under Kästner, a distinguished mathematician in his day. After graduating at Göttingen he proceeded to Vienna, where he took a post-graduate course. During his student career, however, he did not neglect astronomy. In 1779 he devised a new method of calculating the orbits of comets; and in 1781, while in Vienna, he rediscovered Uranus as it emerged from the twilight. Towards the end of 1781 he set up in medical practice in Bremen, and continued as a general practitioner until 1822, when he retired to devote himself exclusively to astronomy. How he continued to lead so strenuous a life must remain a mystery. He was a most conscientious practitioner, and the time given to science was not at the expense of his profession. His little observatory was erected on the upper flat of his house in the Sandgasse, in Bremen, and his largest instrument was a $3\frac{3}{4}$ in. refractor. He never slept more than four hours and yet he lived to the age of eighty-one.

Olbers is chiefly remembered in astronomy by reason of his discovery of asteroids and his work on comets. As far back as Kepler's time it was noticed that there was a big gap in the Solar System between the orbits of Mars and Jupiter; and, indeed, Kepler went so far as to say that a planet might one day be discovered there. The subject first attracted widespread notice, however, when Johann Elert Bode, afterwards director of the Berlin Observatory, drew attention to

a remarkable numerical relationship between the distances of the planets. This has since been known as Bode's Law. If the number four be added to each of the numbers 0, 3, 6, 12, 24, 48, 96, and 192, the resulting series represents pretty approximately the distances of the planets from the Sun, thus—4 (Mercury), 7 (Venus), 10 (Earth), 16 (Mars), 28 —, 52 (Jupiter), and 100 (Saturn). After the discovery of Uranus, it was found that it corresponded to the number 196; and so the fact that the number 28 had no planet to represent it seemed the more mysterious. Bode concluded that a planet existed to fill this gap in the series, and in conjunction with von Zach of Gotha, laid plans for its discovery. Bode and von Zach summoned a congress of astronomers which met in 1800 at Schroter's observatory in Lilienthal, and it was resolved then and there that twenty-four astronomers should take part in a careful search, and to each of them was assigned a zone. One was assigned to the Italian astronomer, Piazzi of Palermo, who was not present at the meeting.

While still unaware that he was expected to co-operate in the search, the missing planet swam into Piazzi's ken. On 1 January 1801, the first night of the nineteenth century, while engaged on the observations for his important star-catalogue, he noticed a strange object which on the next two evenings had altered its position. He decided that he had detected a tailless comet. He hastened to inform Bode of his discovery, and the Berlin astronomer at once concluded that the object of his special search had been found. By this time, however, the body was lost in the Sun's rays, and there were grave fears lest it would not be seen again, for an orbit had never been calculated before from observational material so scanty. At this point Carl Friedrich Gauss, the celebrated Gottingen mathematician, then a young and obscure tutor, solved the problem by the method of least squares, his own mathematical device, and assigned to the missing object a place in the constellation Virgo. There, on 31 December 1801, it was

seen by von Zach at Gotha, and the following evening by Olbers at Bremen. The minute body thus definitely discovered was named Ceres.

The gap had been filled. True, the new planet was disproportionately, even ridiculously, small, and quite invisible to the unaided eye. But before astronomers had time to speculate on its nature or origin, general surprise was created by the announcement by Olbers that on 28 March 1802 he had detected a second object of the same type. Gauss demonstrated beyond a doubt that the second body, which its discoverer named Pallas, was moving in an orbit almost similar to that of Ceres, at practically the same distance from the Sun. This quite unexpected discovery gave rise to the idea that there might be other objects of the same kind yet to be found—an idea justified by the discovery of a third, Juno, by Harding at Lilienthal on 2 September 1804, and of a fourth, Vesta, by Olbers, after three years' careful search, on 29 March 1807. After discovering Pallas, Olbers sought to explain why there were two planets instead of one in the trans-Martian gap, and put forward the theory that the two small bodies were fragments of a larger planet which had been shattered to pieces by a violent explosion in the past. The discovery of Juno and Vesta confirmed him in his provisional hypothesis; but the weight of astronomical opinion has always been against it. Nevertheless, in these days, when cosmogony is indeed in a fluid state, it would be rash to say that this theory may not be rehabilitated.

But Olbers' work on the asteroids was merely incidental to his long-sustained study of comets. For over half a century he swept the skies for these objects. His chief discovery was the comet of 1815, which turned out to be a long-period comet—of the Halley family—with a period of every seventy years. Olbers' comet, as it is called, returned to perihelion in 1887. His chief work in cometary astronomy was on the great comet of 1811. His careful study of this

object led him to propound his theory of comets' tails. Before his time astronomers were 'all at sea' on this question, and Olbers' work marked an epoch. He concluded that the tails of comets were simply streams of minute particles driven out from the Sun by a mysterious 'repulsive force' resident in the Sun. The velocity of the particles expelled from this comet he took to be equal to that of light. Olbers, of course, knew nothing of radiation pressure, and his suggestion of electricity as the prime agent in tail-formation was a natural suggestion. His theory of the constitution of comets' tails was immediately accepted, and never again questioned. It was fitting that it was propounded by the chief cometary authority of the day.

Olbers dealt also with cosmological problems. He was the first to put forward a theory of the extinction of light in space. In 1823 he showed that if the number of stars in the sky were infinite, the whole sky would shine with the brightness of the Sun. Here was a dilemma. Either the universe was finite or light was dimmed in its journey through space. Evidently he believed the one Stellar System to represent the Universe, perhaps misunderstanding Herschel as Struve did. And so he chose the view that the light of the distant stars was extinguished by the ether of space.

Olbers died on 2 March 1840, active to the last. He was a man of lovable disposition, a good companion, kind, generous, and benevolent. A certain unaffected humility was the outstanding trait in his character. Near the end of his life he declared that his greatest service to astronomy had been, not his asteroidal or cometary discoveries, but his discovery of a greater astronomer than himself: he had, he said, discerned, directed, and promoted the genius of Bessel.

Friedrich Wilhelm Bessel was born at Minden, on the Weser, on 22 July 1784. His father was a civil servant of the lower grade, his mother a minister's daughter. He was one of a family of nine, and his parents, though they did nobly by

their family, were unable to provide them with the higher education. Young Friedrich's excellence in arithmetic led his father to apprentice him to Kuhlenkamp & Sons, a commercial house in Bremen.

At thirteen, the same age as his master and friend Olbers, he became interested in astronomy, finding out for himself that Epsilon Lyrae was a double star. On his star-map it appeared as one star, while his uncommonly keen eye divided it. His interest in astronomy did not in any way interfere with his keenness for a commercial career. He sought to fit himself for a post as supercargo on one of the Bremen steamers, and doubtless he believed he would be able to follow out astronomy on board ship. All his leisure time was given to astronomy and mathematics. He constructed a sextant, and began to make observations of his own. At the age of twenty-one he made an original contribution to astronomy. From some old observations of Halley's comet as far back as 1607 he redetermined its orbit. He thereupon forwarded his paper to Olbers. That great man was amazed at the youth's mathematical powers, and urged Bessel to devote himself to astronomy. On the expiry of his seven years' apprenticeship in Bremen, he was introduced in the following year by Olbers to Schroter, whose assistant Harding had just gone to Gottingen. Schroter appointed him to the vacant post, where he remained for four years until in 1810, at the unprecedentedly early age of twenty-six, he was appointed Professor of Astronomy at Königsberg and director of the new Observatory there. It was at Konigsberg that his life-work was to be done. Of the Königsberg Observatory it has been said that 'it will ever remain a monument to his glory, no less than to the munificence of the sovereign who, amid the alarms of war and the desolation of his country, still mindful of science, ordained its institution'.

Bessel's main work was in practical astronomy. The Observatory of Königsberg came into possession of one

of the instruments constructed by another young genius, Fraunhofer of Munich. This was the famous Fraunhofer heliometer, which might be more accurately termed the divided object-glass micrometer. This was the finest instrument of precision ever constructed.

In 1819 Bessel had published his reduction of Bradley's observations made at Greenwich from 1760 to 1762—their positions being corrected for precision and brought up to date. Further, between 1821 and 1833 he made 75,000 observations of other stars; and as a result of his work on Bradley's stars and on others, the number of accurately known star-positions was increased to over 50,000. Here in later years he had the assistance of Argelander, whom he diverted from commerce to astronomy, even as Olbers had diverted him.

Bessel will, of course, be chiefly remembered for his success in measuring the parallax of a star. His attack on the faint fifth-magnitude star numbered 61 in Cygnus was one of three simultaneous attacks. The others were made by Struve and Henderson. Struve's value for Vega was wide of the mark. Henderson's observations on Alpha Centauri were actually made before those of Bessel; but Bessel carried through the reduction of his observations more rapidly than his Scottish friend and co-worker, and in December 1838 he announced that he had succeeded in getting a reliable measure of star-distance. He chose 61 Cygni as the object of his investigation not on account of intrinsic brightness or importance, but because of its large proper motion, which he correctly interpreted as due to comparative proximity. His parallax value indicated a distance of 40 billion miles, a result substantially confirmed by later astronomers. Two months later Henderson announced his value for Alpha Centauri. So far from Bessel feeling piqued by the fact that another astronomer had 'gone halves' with him, the Scottish astronomer's feat filled him with admiration, and the friendship between them grew if anything warmer.

'Light is no real property of mass,' Bessel remarked in a letter to Sir John Herschel in 1844. 'The existence of numberless visible stars can have nothing against the existence of numberless invisible stars.' Bessel based his generalization on the fact that the stars Sirius and Procyon were moving very irregularly, as if influenced by unseen bodies. Accordingly he suggested that each of the stars had a dark companion which disturbed its motion. In this speculation the astronomer was in advance of his time, and little attention was paid to his opinion. His theory, however, proved to be correct, as the satellite of Sirius was discovered in 1862 and that of Procyon in 1896. The Sirian satellite has recently been found to be one of a rare class of stars—the 'white dwarfs'.

Had Bessel been spared to old age he would have almost certainly discovered the planet Neptune. He had fully convinced himself that the irregularities in the motion of Uranus were due to the pull of an exterior planet, and laid his plans in 1840 for an attack on the problem which would have led to the finding of the disturbing body. But the attack was never made. In 1841 he suffered a cruel bereavement in the death of his only son, an astronomer of promise, who seemed destined to carry on his father's work. The son's death marked the end of the father's career. He had now no heart to tackle the Uranian problem. He sank under the blow and died at Königsberg, on 17 March 1846, aged sixty-one.

We now turn to yet another distinguished astronomer who began as an amateur. **Heinrich Samuel Schwabe** was born at Dessau, in Saxony, on 25 October 1789. Like Bessel, he was an official's son. He chose, as a mere boy, the profession of apothecary, and after elementary education in his native town he proceeded to the University of Berlin, where he took a science course. Returning to Dessau he set up as an apothecary. But like Schröter and Olbers he had an interest other than his professional work. His professional work led him to study botany; and as a youth he had been led to the

study of anatomy. His early years in business did not give him much opportunity for original work, but in 1826, at the age of thirty-seven, he decided to give his leisure to astronomy. He sent to Munich for a small telescope, and with this he began to observe the Sun. His instrument, though of first-class quality, was not powerful, and his friends probably smiled at him. He seemed merely to be amusing himself.

Every clear day Schwabe pointed his telescope at the Sun and counted the number of visible spots—a pastime which he continued for forty-three years. After seventeen years' observations had been accumulated, Schwabe was struck by a certain periodicity in the number of spots visible per day. By 1851 what he had suspected was confirmed beyond all doubt: the number of sun-spots passed through a cycle, increasing and decreasing in about ten years. No one was more surprised at this first-class discovery than Schwabe himself. He compared himself to Saul, who, seeking his father's asses, had found a kingdom.

After Schwabe had detected this periodicity, which earlier observers had declared to be non-existent, Rudolf Wolf of Zurich, the erudite historian of astronomy, searched through all available records of sun-spot observations, from the time of Galileo and Scheiner downwards, in order to check Schwabe's results. The discovery of the obscure apothecary was abundantly confirmed. Wolf's investigation enabled him to determine the average length of the solar cycle, more accurately than Schwabe had done, as 11·11 years. Additional interest was given to this discovery by the simultaneous discovery by Lamont, a Scotsman naturalized in Germany, who presided over the Munich Observatory, that terrestrial magnetic variations obeyed a somewhat similar period. Thus the solar cycle was shown to have a terrestrial counterpart, and the presumption was very strong that this was no coincidence.

Schwabe did not confine himself to the Sun alone. He made incursions from time to time into other branches of

astronomy. But the day-star remained his first love, and he continued to observe it till old age and infirmity came upon him. He died at Dessau on 11 April 1875, in his eighty-sixth year.

Johann Franz Encke was born at Hamburg on 23 September 1791. Like Olbers, he was a minister's son. His father died when he was only four years of age, and as he was the second youngest of a family of eight, it may be understood that his widowed mother had a very severe struggle. At an early age young Johann Franz gave promise of great ability, and he had the good fortune to come under the notice of a clever mathematician named Hipp, who not only tutored him but rendered him and his widowed mother much financial assistance. The result was that Encke was enabled to enter the high school at Hamburg, where he distinguished himself in Latin and Greek as well as in mathematics, and in October 1811 he had the pleasure of enrolling at Gottingen as a student under Gauss.

His studies were interrupted by the invasion of Germany by Napoleon—that invasion which destroyed Schroter's observatory and broke his heart. 'Neither Gauss nor astronomy could retain the young student at his books,' it was said of him, 'and obeying the impulse which animated the whole heart of Germany in the spring of 1813 he took up arms and marched to Hamburg for the rescue of his country from the French.' It was not until after Waterloo that Encke was free to return to astronomy. On completing his studies at Göttingen he became in 1817 assistant, in 1820 vice-director, and in 1822 director of the Observatory at Seeberg near Gotha. His years there were years of great activity. His first outstanding piece of work has been called 'the greatest step that had been made in the astronomy of comets since the verification of Halley's Comet in 1759'.

On 26 November 1818 a faint telescopic comet was discovered by Pons at Marseilles. Encke suspected that it was

identical with comets observed by a Frenchman named Méchain in 1786, by Caroline Herschel in 1795, and by Pons in 1805. He calculated the elements of its orbit, and arrived at the remarkable conclusion that it revolved round the Sun in a period of three years and a quarter, and he predicted that its next perihelion passage would take place on 24 May 1822. He also stated that, on that occasion, owing to the position of the Earth, it would be invisible to observers in Europe. The Observatory at Paramatta, however, was in readiness to test the daring prediction of the German astronomer, and the perihelion passage took place within three hours of the time predicted by Encke. This at once placed the Seeberg observer among the leading astronomers of the day. Indeed, the name of Johann Franz Encke was at once coupled with that of Edmund Halley. In the words of Miss Clerke, 'the importance of this event will be better understood when it is remembered that it was only the second instance of the recognized return of a comet; and that it, moreover, established the existence of a new class of celestial bodies, distinguished as comets of short period.' The comet has ever since been known by Encke's name, although he himself persisted for some time in calling it 'the comet of Pons'.

Encke's work on the transits of Venus of 1761 and 1769 was published at Gotha in 1824. His discussion and reduction of all the observations of these transits enabled him to narrow down the margin of error. His value of 95 million miles for the Sun's mean distance was 2 million miles too great, but it was a very great advance on previous determinations.

In 1825 Encke was appointed on the advice of his friend Bessel to the directorship of the Berlin Observatory. Here he worked for close on forty years. His later work was not so spectacular as his earlier, but his activity was prodigious. He just missed making the actual telescopic discovery of Neptune. Le Verrier had written to Berlin indicating the

position of the planet on the sky. But Encke appears to have been a little sceptical. He received the young Frenchman's letter on the 23rd of September. It was his birthday, and there was to be a celebration in his home circle. He had decided to take the evening off, and accordingly he put his assistant Galle in charge of the search. And so to Galle and not to his chief fell the distinction of picking up the distant planet.

An apoplectic fit in 1859 left Encke's health permanently weakened. Nevertheless he continued in office until 1864, when increasing infirmity forced his retirement. He died at Spandau on 26 August 1865.

Friedrich Georg Wilhelm Struve was born at Altona in Holstein on 15 April 1793. His father, Jacob Struve, was head master of the High School, and well known for his mathematical and classical powers. His mother was the daughter of a Lutheran minister, Stinde by name, who had gone to Russia as chaplain to Tsar Peter III. It was the presence of the maternal grandfather in Russia that determined the future career of young Wilhelm.

He was ready for the University at the early age of fifteen, but his parents decided to send him to Russia. Napoleon was menacing Germany, and his well-known methods of overrunning a country and conscripting its young men to fight in his battles filled Struve's parents with dread. And so that same horror of militarism which caused William Herschel's parents to send him to England impelled Wilhelm Struve's to send him to Russia. His elder brother was a lecturer at Dorpat, in Estonia, and it was to that University that young Struve was sent in 1808. It was his father's desire that he should first of all study philology, and he took his degree in that subject in 1811, at the age of eighteen. He then passed to the study of science, and in 1813 he took his degree as Doctor of Philosophy. He took astronomy in his science course, and while a student he was allowed free access to the

Observatory, then rather poorly equipped with instruments. The professor was at that time in a precarious state of health, and, on his death in 1815, Struve, then only twenty-two, succeeded to the chair, which he occupied for thirty-six years.

In 1824 a fine 9-inch refractor by Fraunhofer, driven by clockwork, was procured for the Observatory, and with this instrument he continued his work on double stars, which had been commenced soon after his appointment as director. His first catalogue of double stars was published in 1822, but his chief work did not begin until the great refractor was installed. Between 1824 and 1827 he passed in review 120,000 stars, and this survey resulted in the discovery of 2,200 new pairs. The results of his work were embodied in three great treatises, which take rank as standards on the subject.

Towards the close of his period at Dorpat he attacked the problem of stellar parallax, independently of Bessel and Henderson. He chose the bright star Vega as the object of his attack, and from measures made in the years 1833 to 1838 deduced a parallax of 0·261″. This parallax inspired less confidence than Bessel's or Henderson's measures, and was afterwards found to be considerably in error.

In 1839 Struve was called to direct the new Imperial Observatory at Pulkova, near St. Petersburg (now Leningrad). This great institution was for many years the last word in astronomical efficiency, and Struve, then at the zenith of his powers, filled the office of director for twenty-five years. The chief instrument was a refractor of 15 inches aperture, with which he again returned to the study of double stars. By this time he had the assistance of his son Otto, a young man in his early twenties, and a survey carried through by father and son resulted in the discovery of 514 new pairs. In the later 'forties Struve turned his attention to the problem of the structure of the Universe, and his book *Études d'Astronomie Stellaire*, published in 1847, may be called the first serious contribution to cosmology since the time of

Herschel. Struve was the first astronomer who made a close study of Herschel's papers, and he concluded that Herschel had abandoned the disc-theory.

Struve's own researches had led him to regard the Stellar System—consisting of all the stars, clusters, and nebulae visible in the most powerful telescope—as of finite thickness but of infinite extension in the galactic plane. He maintained, indeed, that this was borne out by Herschel's later investigations, and that when Herschel spoke of the Galaxy as fathomless he meant that it was unfathomable. Along with this view of an infinite extension in the galactic plane, Struve advanced his theory of the extinction of light in space, maintaining that the more distant of the galactic star-clouds were rendered invisible by this extinction. Struve's hypothesis was rejected by the majority of his contemporaries, and their rejection has been justified by subsequent research. Sir John Herschel showed that in important respects it failed to explain the observed facts: while Encke pointed out that the theory was based on five assumptions, all of which were questionable.

Struve's health broke down in 1858, but he continued as director at Pulkova until 1861, when he retired in favour of his son Otto Struve, who worthily carried on his father's work. After three years' retirement, Wilhelm Struve died on 23 November 1864, in his seventy-second year.

When Struve exchanged Dorpat for Pulkova he was succeeded by yet another of the band of great German observers. This was **Johann Heinrich Mädler**. Born at Berlin on 20 May 1794, Mädler's early life was a hard one, and only his indomitable perseverance enabled him to surmount the obstacles in the way of his pursuit of a scientific career. His parents, who were in comfortable circumstances, educated him with a view to the teaching profession, and he had decided to study mathematics and astronomy at the newly founded University of Berlin. But the sudden death

of both his father and mother left him at the age of eighteen as the sole support of three young sisters. For several years he had a hard struggle. By day and by night he worked, alternately learning and teaching, with the supreme object of providing for himself and his sisters and of acquiring the preparatory knowledge necessary for the accomplishment of his cherished ambition, to enter on an academic career. At last, at the age of twenty-four, he succeeded in saving enough money to enable him to enter the University, where he studied astronomy under Bode and afterwards under Encke.

In 1822 he obtained a good teaching appointment in Berlin. He was obliged to augment his income, however, by giving private lessons, and it was fortunate for him that this was so. A wealthy banker, Wilhelm Beer, brother of Meyerbeer the composer and Michael Beer the poet, went to him for lessons in mathematics and astronomy. So keen did he become on the latter science that in 1829 he built a private observatory in the grounds of his villa in Berlin, and equipped it with one of Fraunhofer's fine little telescopes of 4 inches aperture. Here pupil and tutor began a series of observations which made them deservedly famous. Two celestial bodies were subjected by them to a careful scrutiny—Mars and the Moon. Mars was studied during five oppositions from 1830 to 1839. Considering the small size of the telescope, the work which they did on Mars was marvellously good. They redetermined the rotation period, delineated the chief surface-markings, and detected for the first time a broad blue band surrounding the shrinking polar cap. The late Dr. Lowell truly said that 'with Beer and Mädler came the first attempt at a complete geography. In and out through the ochre was traced the blue: commonly in long Mediterraneans of shade but here and there in isolated Caspians of colour.'

The work, however, by which Beer and Mädler will be chiefly remembered was that on the Moon. Most of the work was done by Mädler, who was a born observer, but Beer was

an able helper, and in addition bore the cost of the undertaking. From 1830 to 1834 they measured the positions of 919 lunar formations, and the height of 1,095 mountains. Their great lunar chart was issued in four parts during the years 1834–6. It has been said of this chart that 'the amount of detail is remarkable, and the labour actually bestowed on the work will appear incredible'. The chart was followed in 1837 by a descriptive volume entitled *Der Mond: oder allgemeine vergleichende Selenographie*—'The Moon: General and Comparative Selenography'. In this monumental work Beer and Mädler recapitulated the sum of human knowledge concerning our satellite. Their view of the lunar world as changeless, airless, and lifeless was much nearer to the truth than the 'baseless fabrics' of Schröter's visions. At the same time, the publication of this exhaustive volume accompanying an equally exhaustive chart tended to discourage further investigation of the Moon's surface. Beer and Mädler were believed to have ascertained all that was worth knowing, and to have established the fact that the Moon was to all intents and purposes dead. Indeed, the view still survives that it is a waste of time for an astronomer to devote himself to lunar research.

Der Mond took the scientific world by storm. Humboldt, then at the height of his influence, made generous reference to it. Mädler received the honorary title of Royal Professor of Astronomy from the King of Prussia, and was clearly marked out for preferment. Several professorships were offered him; but he bided his time and at last, in the end of 1840, when in his forty-ninth year, he accepted an invitation to succeed Struve at Dorpat. Here he remained for a quarter of a century.

He found the climate of Dorpat unsuitable for his lunar and planetary work, and so he turned his attention to exact astronomy, and to the problems of cosmology. His famous theory of the 'central sun', propounded in 1846, attracted

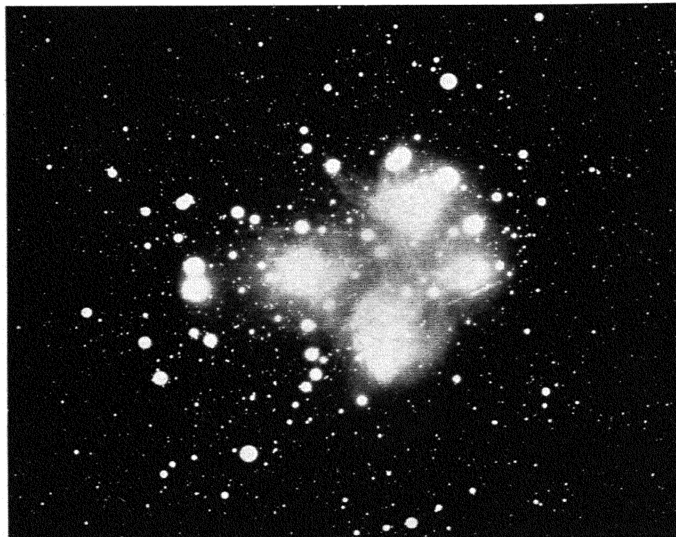
universal attention, and received approval in many popular text-books. The hypothesis was formulated as the result of a very careful investigation. Assuming with Herschel that the Stellar System is a thin flat disc with diameter much greater than its thickness, Mädler concluded that the centre of gravity of such a system must be found somewhere within the limits of the Milky Way and in the northern half of the smaller of the two parts into which the Milky Way divides the sky.

Mädler also concluded that the motions in the Stellar System must be fundamentally different from those in the Solar System. In our system the Sun is the dominating body, and the planets nearest to the Sun consequently move much more rapidly than those most distant. In the Stellar System, on the other hand, the mutual attractions of the different stars would cause the stars at the boundaries of the system to move much more rapidly than those at the centre: indeed, Mädler believed stars at the centre must be practically at rest. He therefore undertook to search for a region of very sluggish proper motions, where the stars would be, as it were, held in equilibrium by the mass of the great stellar multitude. He found such a region in the Pleiades, which he concluded to be 'the central group of the entire system of the stars', and he fixed on Alcyone, the chief star of the cluster, as the 'central sun' whose headship was determined solely 'by its situation at the point of neutralization of opposing tendencies and consequent rest'. Mädler estimated the distance of Alcyone to be 537 light-years and computed the Sun's period of revolution to be 18,200,000 years. The hypothesis was decisively rejected by Struve and the younger Herschel, two of the foremost authorities on stellar astronomy. Struve characterized Mädler's procedure as 'much too hazardous'. And Herschel, while considering that such attempts as Mädler's were 'by no means to be discouraged as forerunners of something more decisive', maintained that the centre of gravity could not possibly lie in the

Pleiades, a cluster twenty-six degrees away from the galactic plane, out of which plane no such general movement of the stars could take place.

Failure of eyesight, together with a longing for his native land, impelled Mädler to retire from Dorpat in 1865, after twenty-five years' service. He was an old man now, but his years of retirement were to be busy years. He planned to round off his career by writing a history of astronomy, and for this purpose he settled first in Wiesbaden and then in Bonn. He was nearly blind when he arrived home in Germany, but an operation to his eyes by a Wiesbaden oculist effected a wonderful cure, and enabled him to proceed with his project. After three years at Bonn he finally settled in Hanover, his wife's native place. Here he completed his *History*, which was published in 1873; and here on 14 March 1874 he died, within two months of completing his eightieth year. He was a man of deep piety as well as wide learning, maintaining that 'a genuine student of nature could not be an atheist'.

Friedrich Wilhelm August Argelander was born in Memel, then incorporated in East Prussia, on 22 March 1799. His father, who was of Finnish descent, was a prosperous merchant of Memel. At the age of eighteen Argelander entered the University of Konigsberg, where he fell under the spell of Bessel. And that great astronomer's influence led him to abandon the idea of a commercial career, for which he was at first intended, and to devote himself to astronomy. In 1820, at the age of twenty-one, he became Bessel's assistant, and collaborated with him in one of his surveys of the heavens. Here he remained for three years, during which he gained his doctorate for a thesis on Halley's comet. In 1823 he was invited to go to Finland as director of the Observatory at Abo, then the capital of Finland. A great fire in 1827 destroyed the University, and although the Observatory escaped, it was decided to transfer it as well as the University



THE PLEIADES

Photographed by Max Wolf

to Helsingfors, the new capital. Argelander equipped the new Observatory with a Fraunhofer refractor, with which he proceeded to accurate determinations of the positions and motions of the brighter stars. Towards the close of his residence in Finland he published his famous work on the solar motion, in which from a study of the proper motions of 390 stars he confirmed beyond doubt Herschel's conclusion, reached in 1783, that the Sun was moving towards the constellation Hercules—a conclusion which had been questioned by most of the leading astronomers, including Herschel's own son.

In 1836 a new Observatory was erected in connexion with the University of Bonn, and the Prussian Government invited Argelander to accept the post of director. Warmly welcoming the opportunity to return to his native land, he accepted, and was installed in his new office in 1837. Bonn was now to be his permanent residence, and to be imperishably associated with his labours. Before the Observatory was properly equipped, Argelander devoted himself to the determination of stellar magnitudes and the study of variable stars. He may be said with justice to have been the founder of this branch of the science. But this was merely preliminary to his greatest work, the *Durchmusterung*, a survey of all the stars of the northern hemisphere down to the ninth magnitude. The whole sky was divided into zones, and each zone was swept by his 3-inch refractor—the position and magnitude of each star being carefully noted. This census of the sky, the most exhaustive hitherto attempted, contained 324,198 stars. In this he was helped by able assistants, chief among whom was Eduard Schonfeld, who latterly succeeded him at Bonn and extended his survey to the southern sky. Along with the *Durchmusterung* Argelander prepared a great star-chart, with the positions and magnitudes of each of the 324,198 stars. This great undertaking was completed in 1863. The *Bonn Durchmusterung* remains the

standard census of the brighter stars, and a monument to the genius and perseverance of Friedrich Wilhelm August Argelander.

The great astronomer remained in harness till his death. Of a robust constitution, he enjoyed excellent health until the summer of 1874, when an attack of typhoid fever lowered his vitality. He died at Bonn on 17 February 1875, in his seventy-sixth year. The spirit of scientific investigation Argelander himself characterized as the 'manly spirit which rises in godlike power for the seeking out of truth and the Eternal'. That spirit possessed and dominated him, and he worthily played his part in what he himself called 'the increase of human knowledge' and the investigation of 'the eternal laws which announce . . . the almighty power and wisdom of the Creator'.

We now turn from the German school of astronomers to the one great Scottish observer in the period under review. **Thomas Henderson** was born, a tradesman's son, in Dundee on 28 December 1798, four years after Mädler and three months before Argelander. Educated at the local schools, Henderson learned mathematics from the principal of Dundee Academy, who described him as 'remarkable for everything that was good'. His elder brother was in partnership with a Dundee lawyer, and when in his sixteenth year the future astronomer entered their office, where he was employed in classifying the burgh records. At this time he became attracted towards science. Although of a very delicate constitution, at times almost blind, he studied astronomy during his leisure, and became familiar with the elements of the science.

At the age of twenty-one Henderson left Dundee for Edinburgh, where he placed himself under a Writer to the Signet. Through the influence of Sir James Gibson-Craig he was appointed advocate's clerk to Lord Eldin, and from 1819 to 1831 he filled the post of secretary to the Earl of Lauderdale and Lord Jeffrey. While in Edinburgh he was introduced

to Professors Leslie and Wallace and Captain Basil Hall, and he became a member of the Astronomical Institution of Edinburgh, to which the Calton Hill Observatory, founded in 1776, was at that time attached; and as the young astronomer was permitted to use the instruments there he soon became an apt observer. His visits to London with the Earl of Lauderdale were the means of introducing him to various men of science, including Dr. Thomas Young, to whom he forwarded a method of calculating occultations of the celestial bodies, which was published in the *Nautical Almanac*. In 1828 Henderson became a candidate for the Professorship of Astronomy in the University of Edinburgh, vacated by the death of Dr. Robert Blair. Although supported by Dr. Thomas Young he was unsuccessful, and the Chair of Astronomy remained vacant for fourteen years—a fact which indicates the backward state of the science in Scotland at that time.

Fearon Fallows, 'His Majesty's Astronomer' at the Cape of Good Hope, died in 1831. Henderson was offered the post, which he accepted; and he arrived in South Africa in April of the following year. His two instruments were a 10-foot transit instrument and a defective mural circle; yet with these he did much good work. In the words of Sir David Gill:

'He gave to the world a catalogue of the principal southern stars of an equal accuracy with the work of the best observatories of the northern hemisphere, and which will in all time be regarded as the true basis of the most refined sidereal astronomy of the southern hemisphere. His observations gave by far the most accurate determination of the Moon's parallax then available; they determined the longitude of the Cape with a precision which refined modern methods, with the aid of the electric telegraph, have barely changed. Above all, Henderson was the first man to produce reliable evidence of the measurable parallax of any fixed star,'

an undertaking which had baffled even Bradley, the greatest practical astronomer of the previous century.

Dr. A. W. Roberts says of Henderson: 'He was a born astronomer, and with that instinctive perception which guides such men to the selection of the best means to secure an end—an end as yet elusive and undefined—he set about making a series of observations on Alpha Centauri.' This star, in the constellation Centaurus, is one of the brightest in the heavens, being surpassed only by Sirius and Canopus. Its brilliance and large proper motion led Henderson to the belief that its parallax might be measurable, and he therefore made a series of observations extending over a year. His one assistant was Lieutenant Meadows, and between them they made five or six thousand observations on the positions of the southern stars, and observed the comets of Encke and Biela.

Henderson's observations on Alpha Centauri were actually made in 1832 and 1833, and were therefore, in point of time, in precedence of those of Bessel, which were made in 1837 and 1838. But Henderson worked in the face of many obstacles. The South African climate told adversely on his delicate constitution and he was obliged to resign his post and return to Scotland in 1833. Settling in Edinburgh he devoted himself to the reduction of his observations at the Cape. Then came the preferment which he so richly deserved and which relieved him of financial anxiety. On 1 October 1834 he was appointed Professor of Astronomy in the University of Edinburgh. By this time the Astronomical Institution had handed over the Calton Hill Observatory to the University, and Henderson received the title of Astronomer Royal for Scotland, being the first holder of that office.

On his return to Edinburgh, Henderson found that his observations in South Africa on the star Alpha Centauri showed that its parallax was measurable, and that the distance was about 20 billions of miles. On 3 January 1839 he announced to the Royal Astronomical Society that he had succeeded in measuring the parallax of Alpha Centauri. But he could no longer claim priority, for Bessel, as has been already

stated, had announced two months earlier that he had succeeded in measuring the distance of 61 Cygni; and at the same time the elder Struve made a similar announcement in regard to Vega. Still, the fact remains that Henderson's observations were made before those of Bessel, and under much less favourable circumstances; for, while Bessel had at his disposal the instruments constructed by Fraunhofer, the telescopes used by Henderson were not of the first quality.

As Sir David Gill remarked:

'In the years 1835–1840 the two great masters of practical astronomy, Bessel in Germany and Struve in Russia, devoted themselves to the problem, and finally produced evidence, each in the case of different stars, of a really measurable parallax. But whilst those great masters had been exhausting the resources of their skill in observation, and that of the astronomical workshops of Europe in supplying them with the most refined instruments for this purpose, a quiet, earnest man had been at work at the Cape, and had really made the first observations which gave decisive evidence of the measurable distance of a fixed star.'

The high reputation of Bessel and the accuracy of the magnificent instruments with which he worked at Konigsberg sufficed to silence all scepticism as to the reality of the discovery. In 1842, accordingly, Bessel received the Gold Medal of the Royal Astronomical Society. In the course of his address, the president, Sir John Herschel, referred to Henderson in a somewhat patronizing way: 'Should a different eye and a different circle continue to give the same result, we must, of course, acquiesce in the conclusion, and the distinct and entire merit of the first discovery of the parallax of a fixed star will rest indisputably with Mr. Henderson.' Maclear, Henderson's successor at the Cape, confirmed the results of the great Scottish astronomer; and Gill, in more recent years, likewise confirmed the accuracy of Henderson's observations.

Henderson held the office of Astronomer Royal for ten years.

Although much of his time was taken up with reducing his Cape observations, he made during these years, and with only one assistant, 60,000 observations, chiefly on the planets and on the stars belonging to the zodiacal constellations; and he also computed the orbits of several comets.

As already mentioned, however, he suffered throughout his life from the most delicate health. It is therefore not surprising to learn that he died at an early age. The death of his wife in 1842 was a blow from which he never recovered. The following summer, however, he enjoyed a trip to the Highlands in the company of Bessel and the mathematician Jacobi. Until a month before his death he continued his observations. In fact, he carried on his work until illness made the ascent of the Calton Hill impossible for him. He died in Edinburgh on 23 November 1844, within a month of completing his forty-sixth year.

The measurement of the distance of the nearest star was followed in less than seven years by the discovery of what was reckoned until a few years ago the remotest planet. The story of the discovery of Neptune has often been told, but it is well to recapitulate briefly the steps which led up to it, as a preliminary to what has to be told of its discoverers, Le Verrier and Adams.

Soon after the discovery of Uranus by Herschel, the mathematicians busied themselves with calculating its orbit. Bouvard, co-operating with Laplace, published tables of the planet, based not only on observations made since 1781 but also on observations made by astronomers who had erroneously considered Uranus to be a star. It soon became apparent that there was a discrepancy between theory and observation. Uranus did not move according to Bouvard's predictions. He therefore rejected the earlier observations altogether, and based his new tables on scantier but more reliable data. His predictions, however, were again falsified. The irregular motion continued. Certainly the discrepancy be-

tween theory and observation was small, judged by everyday standards. If the real and theoretical Uranus had been placed side by side in the sky, they would have seemed a single planet, even to the keenest eye. But a divergence of two seconds of arc was intolerable to the mathematicians of the nineteenth century.

Bouvard in the first place threw out the suggestion that the irregularities were due to an unseen perturbing body. Mädler made a similar suggestion, and the conviction grew on the scientific world that this was indeed the case. Bessel, one of the first mathematicians of the day, formed the design of finding out from the observed perturbations the position of the unseen planet in the sky; but before he was able to attack the problem he was stricken by fatal illness. Meanwhile a Cambridge undergraduate noted in his diary in 1841 his resolve to investigate 'the irregularities in the motion of Uranus, which are as yet unaccounted for, in order to find whether they may be attributed to the action of an undiscovered planet beyond it: and if possible thence to determine the elements of its orbit approximately, which would lead probably to its discovery.' Independently of this young Englishman, a young Frenchman undertook some years later the same task. We shall now sketch briefly the careers of these two distinguished men.

Urban Jean Joseph Le Verrier was born at St. Lô in Normandy on 11 March 1811. He was the son of an obscure Government official, who was, however, able to provide a good education for the future mathematician. Le Verrier was educated at the École Polytechnique in Paris, where he first gave evidence of his great mathematical powers. At the outset of his career he decided to adopt civil engineering as his profession, and for that purpose began to make laboratory experiments in chemistry. But he was not destined to become a civil engineer. His mathematical powers were so well known that he was offered, and accepted in 1837, the post of

astronomical teacher at the École Polytechnique. At the age of twenty-eight he began his investigations in mathematical astronomy, and in 1839 forwarded to the Academy of Sciences two papers on the stability of the Solar System. These papers placed Le Verrier right in the front rank of astronomers.

John Couch Adams, who was to share with Le Verrier the honours of the great discovery soon to be made, was born at Lidcot in Cornwall on 5 June 1819. A farmer's son, he received his early education in a private school at Devonport, conducted by his mother's cousin, an Anglican clergyman. While still a schoolboy Adams developed an interest in astronomy, and his leisure time was devoted to reading all the astronomical books on which he could lay his hands. At twenty years of age he entered Cambridge University, and while still a student there he resolved, as already stated, to tackle the problem of the irregular motion of Uranus.

After two years' work he came to the conclusion that a planet revolving at a certain distance beyond Uranus would account for the observed irregularities, and he handed to Challis, the director of the Cambridge Observatory, the elements of what he called the 'new planet'. On 21 October 1845 he called at Greenwich Observatory and left for the Astronomer Royal a paper containing the elements of the unseen planet and a determination of its position in the sky. The Astronomer Royal was a busy man and evidently was not impressed. He wrote to Adams, however, asking a question which he felt to be of considerable importance. Adams, either by reason of negligence or pique, failed to reply, and so Airy took no steps to search for a planet in which he did not really believe. An unfortunate accident, moreover, prevented the discovery being made elsewhere as a result of Adams' work. Dawes, the celebrated English observer, saw Adams' papers at Greenwich, and was so impressed with them that he wrote to his friend Lassell, then in possession

of probably the finest reflector in England, asking him to search in the portion of the sky indicated by the work of Adams. Unfortunately, Lassell was suffering from a sprained ankle, and his friend's letter was accidentally destroyed by a careless housemaid.

Meanwhile the perturbations of Uranus had been attracting the attention of the French astronomers. Arago, the versatile director of the Paris Observatory, convinced of the existence of an exterior planet, urged on his young friend, Le Verrier, the advisability of an attack on the problem. Ignorant of the fact that Adams had already solved it, Le Verrier set about his task in 1845. In three memoirs communicated to the French Academy he demonstrated firstly, that the perturbations of Uranus could not be produced by any known cause; secondly, that an exterior planet alone could produce them; and thirdly, that the exterior planet would be found in a certain place in the constellation Aquarius. One of these papers happened to come under the notice of Airy. Noticing that Le Verrier had reached a conclusion similar to Adams, he became at last convinced of the urgency of the question, and wrote to Challis of Cambridge, suggesting that a search should be made in the constellation Aquarius. In July 1846 Challis began this search. The planet was actually seen on the 4th and 12th of August, but was not recognized. 'After four days of observation', Challis wrote to Airy, 'the planet was in my grasp if I had only examined or mapped the observations.' Airy had been too late.

In the meantime Le Verrier had not been idle. On 18 September he wrote to J. G. Galle, then chief assistant to Encke at the Berlin Observatory, asking him to search for the planet in the constellation Aquarius. Galle passed on the letter to Encke. That great man, as already stated, had up to now been somewhat dubious as to the existence of a trans-Uranian planet, and he was not too anxious that time should be spent on this quest. In any case, it was his birthday and he

intended spending the evening in the family circle. However, he gave permission to Galle and a younger assistant, D'Arrest, to examine the region of the sky indicated by Le Verrier. Fortunately, Bremiker's star-chart had just arrived at the Observatory. Galle and D'Arrest made use of it. D'Arrest put it on a desk and verified the stars which Galle announced from the telescope. Then they came on an object which was not on the map and they at once went for Encke, who somewhat reluctantly left his birthday party to take part in the later observations.

'The same evening', wrote Encke in a communication to Schumacher, the editor of the *Astronomische Nachrichten*, 'Galle compared with the sky the excellent maps which Dr. Bremiker has plotted . . . and he almost immediately noticed very near to the position which Le Verrier predicts a star of the eighth magnitude which was missing on the chart. It was immediately measured three different times by Galle with reference to a star in Bessel's catalogue (each measure consisting of five observations) and was once measured by me.'

On the following evening Encke and Galle found distinct traces of motion, and on 25 September, when, in Encke's words, 'Galle compared the star five times and I ten times, the motion was confirmed'. Thus was carried through what Encke called 'the most brilliant of all planetary discoveries'.

Then on 29 September, only six days after the planet was picked up at Berlin, Challis found it at Cambridge, but the priority of the discovery was lost to Adams. A long and unedifying controversy ensued as to the 'rights of discovery', in which, it is pleasant to relate, neither Adams nor Le Verrier took any part.

After the discovery honours were showered on Le Verrier. He received the Grand Cross of the Legion of Honour, and in 1852 was created a Senator of France. On 3 October 1853 Arago died, and the following year Le Verrier was appointed

to succeed him as director of the Paris Observatory, the office in France corresponding to that of Astronomer Royal in England. Although he paid considerable attention to practical astronomy in his new position, he continued his mathematical researches. His success in solving the problem of the irregular motion of Uranus led him to pursue this line of research. In the course of his investigations of the movements of the planets he found that the motion of Mercury was subject to considerable irregularities. So great, indeed, were these irregularities that on 12 September 1859 he announced to the Academy of Sciences that they could be accounted for by the existence of another planet revolving round the Sun within the orbit of Mercury. Immediately after Le Verrier's statement he received a letter from Dr. Lescarbault, a physician at Orgeres, announcing that on 26 March 1859 he had observed an intra-Mercurial planet in transit over the disc of the Sun. Le Verrier himself visited Lescarbault at Orgeres, and being assured that the supposed observation was genuine, he announced the discovery of a new planet, to which he gave the name of Vulcan. He also stated that it revolved round the Sun in a little under twenty days. But it was never seen again. Some years later Le Verrier investigated a list of supposed transits of Vulcan, and fixed 22 March 1877 and 15 October 1882 as probable dates of future transits, but on those dates, although an exhaustive search was kept up, no planet was to be seen. Thus Le Verrier's quest for an intra-Mercurial planet was fated to end in failure. He was more successful, however, in his study of the very smallest members of the Sun's family. He paid much attention to meteors, and published in 1867 the elements of the November swarm, known as the Leonids. He assigned a date (A.D. 126) in which the Leonids were probably introduced into the Solar System through the influence of the planet Uranus.

Le Verrier was of an irritable disposition, and not qualified to

direct a great Observatory like that of Paris. M. Flammarion, who was his assistant from 1858 to 1862, said that Le Verrier's life 'would have been still more useful to science and humanity if he had possessed a more sociable character and a more disinterested love for the general progress'. It is therefore not surprising to learn that disputes arose between the astronomer and his assistants, and after the difficulties had become intolerable Le Verrier was obliged to retire, and was succeeded by an eminent mathematician, M. Delaunay. M. Delaunay was accidentally drowned in 1873, and Le Verrier was again appointed to the post of director, which he held for the remainder of his life.

Just as Lagrange and Laplace lived through the first French Revolution, Le Verrier experienced the third. He was a supporter of the Emperor Napoleon III, and during the uprising known as the Commune he was the object of much popular hatred: indeed, fears were entertained for his personal safety. A few years later his health began to give way, and he died in Paris on 23 September 1877, aged sixty-six years, and was buried on Mont Parnasse.

Le Verrier was not an observer: he was, pre-eminently, a mathematician.

'Very often I submitted to him', said Flammarion, 'the doubts of an anxious mind on the great problems of Infinitude. I asked him if he thought the other planets might be inhabited like ours: what might be especially the strange vital conditions of a world separated from the Sun by the distance of Neptune: what might be the retinue of innumerable suns scattered in immensity: what astonishing coloured lights the double stars should shed on the unknown planets which gravitate in these distant systems. His replies always showed me that these questions had no interest for him, and that, in his opinion, the essential knowledge of the Universe consisted in equations, formulae, and logarithmic series, having for their object the mathematical series of velocities and forces.'

Le Verrier is to be reckoned among the ten or twelve greatest names in mathematical astronomy, and his place in the history of the science is an enduring one.

England was as proud of Adams as was France of Le Verrier. He declined in 1849 the honour of knighthood. In 1858 he became Professor of Mathematics in the University of St. Andrews. His residence in Scotland was not of long duration, for a few months later he became Lowndean Professor of Astronomy and Geometry at Cambridge. In 1861 he was appointed director of the Cambridge Observatory in succession to Challis. His later work was chiefly concerned with the motion of the Moon and the orbit of the Leonid meteors. In 1799 there was a great shower of meteors visible in South America, of which Humboldt left an exhaustive account. The shower recurred in 1833, and Olbers then suggested that such showers were periodic; and the view became general that the shower was due to the collision of the Earth with a meteor swarm revolving round the Sun in an orbit intersecting that of our planet.

An American astronomer, the late Professor H. A. Newton, showed that there were five possible orbits for the meteoric swarm. The first of these was one of thirty-three years, the second a little over a year, and the third a little under a year, while there was also a possibility of two smaller orbits. Adams now occupied himself with the question, and in April 1867 announced that the Leonids revolved round the Sun in a period of thirty-three years and a quarter.

In 1881 Adams was offered the post of Astronomer Royal in succession to Sir George Airy, but he preferred to carry on his chosen work at Cambridge. Eight years later his health broke down, and he died at the Cambridge Observatory on 21 January 1892, aged seventy-two. In his disposition Adams was very different from his great contemporary, Le Verrier. He combined a lovable nature and a brilliant intellect with a cheerful and genial disposition.

In the words of Dr. Glaisher: 'Strangers who first met him were invariably struck by his simple and unaffected manner. He was a delightful companion, always cheerful and genial, showing in society but few traces of his really shy and retiring disposition. His nature was sympathetic and generous, and in few men have the moral and intellectual qualities been more perfectly balanced.'

VI

PIONEERS OF ASTROPHYSICS

JOSEPH FRAUNHOFER—GUSTAV ROBERT KIRCHHOFF—GIOVANNI BATTISTA DONATI—ANGELO SECCHI—WILLIAM HUGGINS—PIERRE JULES CÉSAR JANSSEN—JOSEPH NORMAN LOCKYER—JOHANN CARL FRIEDRICH ZÖLLNER—HERMANN CARL VOGEL—NILS CHRISTOFFER DUNÉR—EDWARD CHARLES PICKERING—GEORGE ELLERY HALE—HENRY NORRIS RUSSELL

'THAT a science of stellar chemistry should not only have become possible, but should already have made material advances, is assured by one of the most amazing features in the swift progress of knowledge our age has witnessed.' So wrote Miss Agnes Clerke, the historian of astronomy, in her monumental history of the science, published in 1886. The rise of astrophysics, or the new astronomy, was indeed the outstanding event of the latter half of the century. In 1823 the philosopher Comte had declared categorically that one secret at least must always be hidden from mankind—the chemical make-up of the celestial bodies. Within less than forty years the secret was out: indeed, even while Comte wrote, and all unknown to him, the foundations of astrophysics had been laid.

And here it is necessary, chronologically speaking, to retrace our steps: for the pioneer of astrophysics was a contemporary of John Herschel and Bessel and Struve, and was indeed yet another luminary in the brilliant constellation of astronomical luminaries whose light shone forth from Germany in the early years of the century. **Joseph Fraunhofer,** the son of a glazier in straitened circumstances, was born on 6 March 1787 at Straubing, in Bavaria. His father and mother died while their son was yet a child, and at the age of fourteen young Fraunhofer was bound as an apprentice to a looking-glass maker of Munich named Weichselberger, a cruel and

tyrannical man who sweated and bullied the delicate lad. Deliverance from this grinding tyranny came about in an unexpected way. On 21 July 1801 the wretched slum tenement in which young Fraunhofer lodged tumbled down. All who at the time were in the building lost their lives except Fraunhofer, who was extricated from the debris seriously but not permanently injured. It so happened that the Elector of Bavaria, driving past in his carriage, witnessed the disaster and saw the lad pulled out from the ruins. He interested himself in the orphan's plight, visited him in hospital, and, learning his tastes, presented him with a considerable sum of money to enable him to pursue the study of optics.

With part of this sum Fraunhofer purchased his release from his cruel employer. The remainder enabled him to buy books and to continue his studies. A kindly scientific man had the bright boy brought to his notice and gave him every encouragement to continue to work at optics, and in 1806 Fraunhofer applied for and obtained an appointment on the staff of the Optical and Physical Institute of Munich. He had the good fortune to come in contact with a senior member of the staff, Pierre Louis Guinand, a French-speaking Swiss from Neuchâtel, who had succeeded in making achromatic lenses of a vastly superior quality than had ever before been constructed. Under the supervision of this master craftsman, Fraunhofer became an expert lens-maker, and the fame of the pupil far exceeded that of the tutor. In 1817 Fraunhofer succeeded in completing what was by common consent the finest refracting telescope in the world. The object-glass had a diameter of 9½ inches and was for some time the greatest in the world. But of even greater significance was the fact that the telescope moved by clockwork to keep pace with the diurnal motion of the celestial sphere. This instrument became known to fame as the Dorpat refractor. At the new Observatory there it was employed by Wilhelm Struve in his intensive work on double stars. A still more perfect example of Fraunhofer's workman-

ship was the Königsberg heliometer—or divided object-glass micrometer—by means of which Bessel succeeded in his attempt to measure the parallax of 61 Cygni. Encke, Mädler, and Schwabe also did their best work with telescopes of Fraunhofer's construction.

Fraunhofer is best remembered, however, by his pioneer spectroscopic work. Concurrently with his work on lenses, Fraunhofer occupied himself with prisms. The study of the spectrum, the rainbow-coloured strip into which sunlight is dispersed after passing through the prism, had languished since Newton first drew attention to it. For many years the spectrum was an object less of study than of curiosity. William Herschel was the first to make a scientific study of the spectrum, and even he missed its characteristic features—the dark lines. These were seen casually by Wollaston, an English physicist, in 1802; but their effective discovery was really made by Fraunhofer. In 1817 he announced what he had found three years previously as a result of his pioneer investigations. 'I saw with the telescope', he said, 'an almost countless number of strong and weak vertical lines which are darker than the rest of the colour-image. Some appeared to be perfectly black. . . . I have convinced myself by many experiments and by varying the methods that these lines and bands are due to the nature of sunlight and do not arise from diffraction, illusion, etc.' Further he found that these lines were characteristic not only of sunlight received direct from the Sun, but from the sunlight reflected by the Moon and planets. 'The light of the Moon', he said, 'gave me a spectrum which showed in the brightest colours the same fixed lines as did sunlight, and in exactly the same places'; and in the spectra of Venus and Mars he identified the same lines.

Fraunhofer then turned his rudimentary spectroscope to the brighter stars and was able to satisfy himself of the existence of lines in their spectra, but he found that many of these differed in respect alike of intensity and position from

the solar lines. Fraunhofer found that each star had its own distinctive spectrum, and yet that some spectra closely resembled others—a kind of forecast of Secchi's discovery of four types of stellar spectra many years later. Fraunhofer was 'unable to perceive any lines in the orange and yellow of the spectrum of the light of Sirius, but a very strong band could be recognized in the green. . . . Castor gives a spectrum similar to that of Sirius, and in spite of the faintness of the light I was able to measure the line in the green and found it to be in precisely the same position as for Sirius.' In the case of Pollux, Fraunhofer recognized numerous but faint lines 'resembling those of Venus'—that is to say, the Sun. He identified the D line in the spectrum of Capella as well as of Pollux, and in the case of Betelgeux he remarked on the dissimilarity of its spectrum from that of the Sun and planets, while noting the existence of lines which seemed to coincide with some in the solar spectrum. These experiments proved conclusively that the dark lines in the solar spectrum and in the spectra of the stars could not be atmospheric in their origin, but were indices of some kind of absorption alike in the Sun and stars. He noted that the bright lines visible in the spectrum of sodium gas coincided in position with the strong dark lines in the solar spectrum to which he had affixed the letter D. His early death, however, put a stop to astrophysical progress for over a quarter of a century.

In 1823 Fraunhofer became Professor in Munich and head of the Optical and Physical Institute. By this time, however, the hand of death was upon him. Always delicate, his early hardships had seriously undermined his health, and symptoms of consumption became apparent in his later thirties. On the eve of a journey to Italy for the benefit of his health he was taken ill, and died on 7 June 1826, in his fortieth year.

Fate decreed, then, that it was not in the Optical Institution at Munich but in the laboratory at Heidelberg that spectroscopic astronomy was to be born. It was **Gustav Robert**

Kirchhoff who discovered the secret of the constitution of Sun and stars.

The life of the discoverer was a singularly uneventful one. Born at Königsberg on 12 March 1824, he was educated at the Universities of Berlin and Marburg. Not astronomy, but physics and chemistry, claimed his attention as a young man, and it was as Professor of Physics that he went to Heidelberg as colleague to the more famous Bunsen. He died at Heidelberg on 17 October 1887, in his sixty-fourth year, twenty-eight years after his greatest discovery. It was in the autumn of 1859, while engaged in the study of luminous gases, that Kirchhoff turned his attention to the Fraunhofer lines, and carried through the experiment which proved to be decisive.

'In order', he said, 'to test in the most direct manner possible the frequently asserted fact of the coincidence of the sodium lines with the lines D, I obtained a tolerably bright solar spectrum and brought a flame coloured by sodium vapour in front of the slit. I then saw the dark lines D change into bright ones. . . . In order to find the extent to which the intensity of the solar spectrum could be increased without impairing the distinctness of the sodium lines, I allowed the full sunlight to shine through the sodium flame, and to my astonishment I saw that the dark lines D appeared with an extraordinary degree of clearness.'

Shortly afterwards Kirchhoff announced the general principles on which spectroscopy is based. A luminous solid or liquid gives a continuous spectrum, and a gaseous substance a spectrum of bright lines. Further, he established the law that substances of every kind are opaque to the precise rays which they emit at the same temperature: they stop the rays which they are in a condition to radiate. The dark lines of the spectrum thus proved to be due to the existence in the solar atmosphere of many of the chemical elements with which we are familiar on this planet. Kirchhoff, at an early stage, announced the existence in the Sun of such familiar elements as sodium, magnesium, iron, calcium, nickel, copper, and zinc.

Kirchhoff's pioneer work acted as an incentive to others, especially to younger astronomers who had not definitely settled on a line of specialized investigation. Huggins recorded that the news of Kirchhoff's discovery was to him 'like the coming on a spring of water in a dry and thirsty land'. Other astronomers shared this feeling, and almost immediately after Kirchhoff had demonstrated the existence of familiar terrestrial elements in the Sun, two Italian observers sought to wrest from the stars the secret of their constitution. These were Donati and Secchi. Donati was the younger of the two, but was actually the first in this field.

Giovanni Battista Donati was born at Pisa on 16 October 1826. Educated at the famous University of his native town, he early turned his attention to science and specialized in astronomy. At the close of his University career he was appointed to the staff of the Observatory at Florence, and in December 1859 was promoted to the directorship, to which office was conjoined that of Professor of Astronomy in the University of Florence.

By this time his name had become famous, through his discovery of one of the finest and most widely observed comets of the nineteenth century. He first saw it on 2 June 1858 as a feeble round nebulosity in the constellation Leo. Steadily brightening as it approached the terrestrial neighbourhood, this comet burst forth into splendour in the autumn, and in late September and early October was, in the words of a competent historian not given to exaggeration—the late Miss Clerke—'the most majestic celestial spectacle of which living memories retain the impress'. The comet not only contributed materially to the advancement of human knowledge concerning these mysterious objects: it incidentally brought its discoverer into fame. After succeeding to the directorship of the Observatory in 1859, he at once turned his attention to the spectra of the stars, and made the first survey of stellar spectra. He succeeded in determining the positions

of the most important lines, but the instrumental means at his disposal were inferior, and he was obliged to abandon his pioneering enterprise.

He was more successful in his application of the spectroscope to comets. He was the first astronomer to examine the spectrum of a comet. A fairly bright comet was discovered by Tempel, a German astronomer resident in Italy, on the 4th of July; and on the 5th of August Donati found its spectrum to consist of three bright bands, yellow, green, and blue. This observation proved that this comet at all events was self-luminous and did not shine simply by reflected sunlight as most astronomers believed. Donati proved that this particular comet, and presumably many others, was composed of gaseous matter excited to luminosity.

Donati died of cholera on 20 September 1873; and it was reserved for another Italian astronomer, eight years his senior, to carry on the work which he had only begun. **Angelo Secchi** was born on 29 June 1818 at Reggio, in the province of the Emilia. From boyhood he was designed for the service of the Jesuit order, and received his education in the Jesuit College of his native town. Even at this stage of his career, however, his horizons extended far beyond mere professionalism. He distinguished himself at college in mathematics and physics; and when in 1848 he left Italy on account of the disturbances there, and after a short residence in England settled temporarily in the United States, he served as teacher of natural science in Georgetown University.

His eminence as a teacher attracted the attention of the Papal authorities, and when in 1849 Di Vico, the director of the Observatory of the Collegio Romano, died, he was recalled from America to superintend the erection of the new Observatory in connexion with that famous institution. It was completed in 1852, and with Secchi at its head became one of the world's most famous observatories. So high a place

did Secchi reach in the scientific world that despite all the troubles in which the Papacy and the Jesuit order were involved, he remained at the head of the Observatory till his death a quarter of a century later. After the discomfiture of the Papacy and the collapse of the temporal power, the Italian Government made special arrangements in order that Secchi might continue to direct the Observatory. He died at the Collegio Romano on 26 February 1878, in his sixtieth year.

Secchi was a skilled telescopic observer, and his work on the planets, particularly Mars, was of a high order. He was also recognized as a leading authority on the Sun and on solar physics generally. But he will be remembered chiefly as the initiator of the first spectroscopic survey of the heavens. In this survey, which occupied Secchi for four years, 4,000 stars were passed in review and their spectra examined and classified, and by 1868 he was in a position to announce that 'all the stars in relation to their spectrum can be divided into four groups, for each of which the type of spectrum is quite different'. He found the first type to be represented by such stars as Sirius, Vega, Altair, Regulus, and Castor. 'The spectra of all these stars consist of an almost uniform prismatic series of colours, interrupted only by four very strong black lines. . . . These lines all belong to hydrogen gas.' Secchi found these first type stars to be very numerous, embracing 'one half of the visible stars of the heavens'.

'Almost the other half of the stars', he said, 'were yellow stars of the second type—such as Capella, Arcturus, Pollux, and Aldebaran, with spectra very similar to the Sun—distinguished by very fine and numerous lines.' The third and fourth types, comparatively few in number, comprised two types of red stars. The third type included such well-known brilliants as Betelgeux and Antares, and Secchi found that 'the spectra of these stars show a row of columns at least eight in number, which are formed by strong luminous bands alternating with darker ones'; he also detected small and fine

lines and concluded that in these stars 'the presence of hydrogen is certain'. The fourth type, comprising a few faint stars fainter than the sixth magnitude, were found to have spectra consisting of 'three large bands of light'. Such was Secchi's famous classification. At first he regarded it as more or less empirical and arbitrary, but latterly he was led to the opinion that it 'represented real physical conditions varied by the temperatures prevailing on the different stars'.

Perhaps the most famous of the pioneers of astrophysics was the great Englishman who for many years held a place all his own among astronomers. **William Huggins** was born in London on 7 February 1824, and received his early education at the City of London School. Instead of following an ordinary university career, he studied various subjects under private tutors, devoting special attention to chemistry and astronomy. For some time his attention was occupied by physiology, but in 1856 he swung back again to physical science and resolved to concentrate on astronomy. In that year he erected a private observatory in the garden of his house at Tulse Hill, London. For some years he devoted himself to some well-trodden fields of observational astronomy, but found little satisfaction therein. Reference has already been made to the enthusiasm with which he hailed Kirchhoff's announcement of the solution of the mystery of the Fraunhofer lines. 'Here at last', said Huggins, writing long afterwards, 'presented itself the very order of work for which in an indefinite way I was looking. . . . A feeling as of inspiration seized me: I felt as if I had it now in my power to lift a veil that had never before been lifted: as if a key had been put into my hands which would unlock a door which had been regarded as for ever closed to man.' As soon as his apparatus had been sufficiently perfected, he commenced his epoch-making work in stellar spectroscopy.

Unlike Secchi, who surveyed thousands of stars and grouped them into classes, Huggins concentrated on certain

selected stars, and made a detailed study of their spectra. Early in 1863 he was in a position to announce the presence of sodium, iron, calcium, magnesium, and bismuth in Betelgeux, and of the same elements in Aldebaran with the addition of tellurium, antimony, and mercury. These were the first of many stars to be individually studied.

But the most dramatic of Huggins' early discoveries was that of the gaseous nature of at least certain of the nebulae. About the middle of the century the tide of astronomical opinion had definitely set against Herschel's theory that many of the nebulae were composed of a 'shining fluid', representing the primeval chaos which would in the course of ages condense into suns and planets. Lord Rosse, who erected on his estate in Ireland the largest telescope in the world, believed himself, erroneously, to have commenced the resolution into stars of the great Orion nebula; and even Sir John Herschel felt constrained to abandon his father's hypothesis. When the spectroscope was invented, Huggins at once realized that the problem of the nebulae was now capable of solution, and on 29 August 1864 he turned his spectroscope towards a planetary nebula in the constellation Draco. Long years afterwards he referred to 'the feeling of excited suspense, mingled with a degree of awe', with which, 'after a few moments of hesitation', he put his eye to the spectroscope. But the suspense was not for long. The spectroscope left the astronomer in no manner of doubt: the spectrum was one of bright lines, showing conclusively that in regard to at least one of the nebulae Herschel had been right. Soon afterwards Huggins probed the secret of the Orion nebula. Here too Herschel was found to be right and Lord Rosse wrong. By 1868 Huggins had examined the spectra of seventy nebulae, and one-third of these were indisputably gaseous. Others, including the Andromeda nebula, gave continuous spectra. This is not surprising, as in the case of the Andromeda nebula and kindred objects we now know

them to be outlying galaxies, whose spectra must of necessity be predominantly stellar.

The Sun, the planets, comets, and temporary stars were all passed in review by Huggins in these crowded and busy years. His experience with the spectroscope was not unlike that of Galileo with the telescope two and a half centuries earlier. Discoveries fell to him. He found, independently of Zollner, that the solar prominences could be observed spectroscopically in broad daylight; he secured evidence of water-vapour in the atmosphere of Mars; and he made extensive observations of the temporary star of 1866 and of later novae. But perhaps his genius was most clearly manifested in his epoch-making work on the motions of the stars. In 1842 Christian Doppler, Professor of Mathematics in the University of Prague, had expressed the view that the colour of a luminous body ought to be changed by its motion of approach or recession just as the sound of a sonorous body is altered. The change in colour, it is true, is so slight as to be imperceptible. What really takes place is a slight shift of the entire spectrum in one direction or another; the spectral lines are moved towards the violet if the source of light is approaching and towards the red if it is receding. Huggins saw that with adequate instrumental equipment it would be possible to measure the radial motions of the stars, that is, the components of their proper motions in the line of sight. The task was a formidable one. 'It would scarcely be possible', said Huggins, 'to convey any true conception of the difficulties which presented themselves in this work from various instrumental causes and of the extreme care and caution which were needed to distinguish spurious instrumental shifts of a line from a true shift due to the star's motion.' Nevertheless he succeeded in getting fairly reliable measures, and he announced in April 1868 that Sirius was receding from the Solar System with a velocity of twenty-nine miles per second in the line of sight: shortly afterwards he stated that Betelgeux, Rigel, Castor,

and Regulus among the bright stars were likewise retreating, while Arcturus, Vega, Pollux, and Deneb gave signs of approach. All this was pioneer work. Photography had not yet been applied to spectroscopy, and visual observations could not yield accurate measures. Other astronomers, with instrumental equipment more adequate than that of Huggins, entered into possession of this new and spacious field of research. But it was Huggins who 'blazed the trail'.

Huggins had made his spectacular discoveries by the middle 'seventies of the century. Spectroscopy had by this time passed out of its first phase, and dramatic advances were to be made by professional astrophysicists at the great observatories which had been equipped for astrophysical study and at the new astrophysical observatories specially erected for the pursuit of this line of research. But it would be an error to imagine that Huggins in his later years 'rested on his oars' or lived on his past reputation. In collaboration with his brilliant and gifted wife, he carried on intensive spectroscopic work almost to the day of his death. Two monumental volumes—*Publications of Sir William Huggins' Observatory*—were issued in 1899 and 1908 respectively, and in these the many researches on which Sir William and Lady Huggins were engaged were fully set forth.

Academic honours were literally showered upon Huggins; and in 1897 he was knighted. These honours, however, sat lightly on him. He was a gentle soul, unobtrusive and kindly; and I shall not readily forget the day when as a very young man I was privileged to see over Sir William Huggins' Observatory, and to be entertained for two or three hours by the kindly old astronomer, well over eighty years of age, and his gracious wife. Sir William Huggins died on the 12th of May 1910, after a very brief illness, at the age of eighty-six. Science lost by his death one of its brightest ornaments and England perhaps its greatest astronomer since Newton.[1]

[1] That is, if we reckon Herschel as a German, which he really was.

Contemporary with Huggins was a Frenchman of great genius, who devoted his life to the new branch of astronomy. **Pierre Jules César Janssen**, son of an eminent musician, was born in Paris on 22 February 1824, a fortnight and a day after his great English contemporary. After completing his University education in Paris, he studied chemistry and physics, and obtained a post first as mathematical teacher and then as Professor of Physics. It was by way of physics, then, that Janssen became an astronomer. He commenced work on the solar spectrum in the early 'sixties, and it is as a student of the Sun that he will go down to fame.

His most dramatic discovery, which brought him into the public eye, was made in 1868. He was sent by the Academy of Sciences to observe the total solar eclipse of 18 August 1868, and was stationed at Guntoor in India. This was the first eclipse since the spectroscope had become the recognized adjunct of the telescope, and Janssen was keen to ascertain the nature of the 'red flames' or prominences. Were they gaseous or not? The spectroscope gave a decisive answer: the prominence spectrum was composed of bright lines, proving conclusively that red flames were genuinely gaseous. During the progress of the eclipse Janssen was struck with the dazzling brilliance of the bright lines, and it occurred to him that by using a high dispersive power and so weakening the ordinary solar spectrum he might see the prominence lines in broad daylight. On the day following Janssen used the necessary dispersive power and was rewarded by seeing the spectra of the prominences. 'I have observed to-day', he said, 'a continuous eclipse.' Further, he found bright lines round the entire 'limb' of the Sun, which observation gave conclusive proof of the existence of a gaseous envelope outside of the photosphere and known as the chromosphere.

This achievement, the honour of which Janssen was to share with his younger English contemporary Lockyer, brought him right into the rank of the world's leading

astronomers. The French Government decided to place him at the head of a new Astrophysical Observatory which was set up at Meudon, in the vicinity of Paris, and here he worked for well over thirty years. The Sun was the object of his chief attention, though other celestial bodies were not neglected. His great Solar Atlas, published in 1904, contained 6,000 photographs of the Sun's surface. Although lame from childhood, Janssen lived to a ripe old age. He died on 23 December 1907, in his eighty-fourth year, two years and four months before his contemporary Huggins.

Joseph Norman Lockyer, who saw the prominence lines in daylight independently of Janssen, was born at Rugby on 17 May 1836. He had in early youth no intention of following a scientific career. He became a clerk in the War Office at the age of twenty-one, and for many years all his astronomical work was done in his leisure hours. While quite a young man he took up observational astronomy and devoted a good deal of time to the study of Mars. But in 1866 he switched over to spectroscopy, as Huggins had done a few years earlier, and commenced spectroscopic work. In that year he came to the conclusion that it would be possible to see the spectra of the red flames of the Sun without an eclipse, and two years later, on 16 October 1868, having obtained a spectroscope of high dispersive power, he succeeded in this aim. Janssen, in India, had made the same discovery two months earlier, but by common consent Janssen and Lockyer have always been reckoned as co-workers and co-discoverers. Independently of Janssen, too, Lockyer found that the prominences were projected from a gaseous envelope surrounding the Sun, to which he gave the name of the chromosphere. In the following year, while studying the spectra of the prominences, he found that one of the lines could not be attributed to any known element. So he called it 'helium' or the 'Sun-element'. It was not until 1895 that it was found to exist upon the Earth.

These were the two dramatic discoveries which stood to Lockyer's credit. But his life was as full and strenuous as that of his greater contemporary Huggins. In 1887 he put forward an elaborate classification of stellar spectra, differentiating between stars with ascending and descending temperatures. This classification was interpreted as an evolutionary sequence, and Lockyer built upon it his meteoritic hypothesis of stellar evolution, which has been called 'perhaps the most comprehensive cosmogonic guess that has ever been attempted'. Lockyer concluded that all self-luminous bodies are 'composed either of swarms of meteorites, or of masses of meteoric vapour produced by heat'. The theory was open to various objections and was rejected by Lockyer's chief contemporaries. Unfortunately his classification of the stars according to temperature received less attention than it deserved; but he lived to see his evolutionary sequence in the main accepted.

In 1881 Lockyer was appointed to a professorship in the Royal College of Science. Four years later he became director of the Solar Physics Observatory at South Kensington, and in 1897 he was knighted. In 1913 he retired to Devonshire, where at Sidmouth he erected the Hill Observatory. He died at Sidmouth on 16 August 1920, in his eighty-fifth year.

We now pass to two of the great German astronomers who turned their attention at this stage to astrophysics. **Johann Carl Friedrich Zöllner** was born in Leipzig on 8 November 1834. After a brilliant career at Leipzig and Berlin Universities, he was appointed Professor of Astronomy at Leipzig in 1872. After ten years' service there he died on 25 April 1882, in his forty-eighth year. He became suddenly famous by his work on the solar prominences. Even before Janssen and Lockyer devised their method of observing the prominence lines in broad daylight, Zöllner had shown theoretically how this could be done. In July 1869, independently of Huggins, he observed the forms of the solar prominences by widening

the slit of his spectroscope, and thus made possible the daily study of the prominences themselves.

In 1865 Zöllner turned his attention to the giant planets, and advanced the theory of their constitution which reigned unchallenged for many years and is to-day accepted by perhaps the majority of astronomers. He drew attention to rapid changes in the cloud-belts of Jupiter and Saturn, which in his view indicated great internal heat. Likewise he pointed to the fact that Jupiter, like the Sun, rotates not as a whole, but in sections: the rotation is accelerated in the equatorial regions, which suggested to him that Jupiter's affinities were with the Sun rather than with our own world.

Zöllner was the first to seek to arrange the spectra of the stars in a presumed evolutionary order. He suggested that yellow and red stars were simply white stars in later stages of cooling. This was the basis of the famous evolutionary sequence associated with the name of his distinguished pupil, **Hermann Carl Vogel.** Born on 3 April 1842 at Leipzig, where his father, Dr. Carl Vogel, was a well-known schoolmaster, Vogel was educated at Leipzig University; he took astronomy in his curriculum, and in 1865, while still a student, he became assistant in the Leipzig Observatory. His decision to follow up astronomy as a profession was due to the influence of Zöllner, and in 1869 he assisted Zöllner in his pioneer work on the solar prominences. In the following year he was appointed director of a private observatory at Bothkamp, in Holstein, where he accomplished some work of the finest quality on the spectra of the planets. In the case of Mars he detected the line of aqueous vapour, thus confirming the earlier work of Huggins. In 1874 the German Government established the new Astrophysical Observatory at Potsdam, and Vogel was offered a post on the staff. He soon became the Observatory's chief man, and in 1882 he was appointed director, which post he held for a quarter of a century.

At Bothkamp he had made visual measures of the radial velocities of the stars, but he rightly concluded that only by the aid of photography could reliable measures be secured. It was not until 1887 that he was able to get good photographs of stellar spectra: in that year he commenced his investigations, which extended over five years. The radial motions of fifty-one stars were measured, and the average speed proved to be about ten miles per second. In the course of these observations Vogel made his greatest discovery—that of a wholly new class of star, the spectroscopic binary. The famous variable star Algol first attracted his attention. For many years it had been suspected that this was really a 'visual variable' only, and that Algol was composed of two stars, the fainter of which periodically eclipsed the brighter. In 1888 Vogel tested this theory spectroscopically and placed it beyond doubt. He found that before each minimum Algol was retreating from the Solar System, while on recovering its brightness its motion became one of approach. Algol, therefore, was shown to be an exceedingly close double star, the centres of the components being separated by only 3,230,000 miles, and not a real variable. In 1890 Vogel found the bright star Spica to be a binary of the same type, but as the plane of its orbit does not lie in our line of sight, it does not suffer variation in light.

Following up the early work of Zöllner, Vogel set out in 1894 to classify the stars more elaborately than Secchi had done and to arrange them in an orderly sequence. He retained Secchi's three main types, but he subdivided them. Type I was divided into three classes. In the first class the metallic lines are faint and fine, and the hydrogen lines conspicuous; in the second, no hydrogen lines are visible; and in the third the hydrogen lines are bright. In 1895, after his assistant, Julius Scheiner, got evidence of the presence of helium in star spectra, Vogel separated his class I*b* from the first type altogether, and designated them as class O, or

helium stars. He divided the second type into two classes, the first comprising the solar stars and the second the Wolf-Rayet stars; and he combined Secchi's third and fourth types into one, his third type. Vogel believed this sequence of stellar spectra to be the order of stellar evolution. The helium and hydrogen stars, he maintained, were the youngest and hottest; while the red stars were 'effete suns, hastening rapidly down the road to final extinction'. Vogel died at Potsdam on 14 August 1907.

Doppler's principle had been applied by Huggins and Vogel to the measurement of stellar velocities in the line of sight. It was reserved for a great Swedish astronomer to apply it to the rotation of the Sun, and to make a discovery of the greatest significance. **Nils Christoffer Dunér** was born at Billeberga, a village in Scania, South Sweden, on 21 May 1839. Educated at the University of Lund, his first interest was in geography, and from 1864 to 1865 he assisted the famous explorer Nordenskiold in his voyages of discovery. In 1864, however, he decided to take up astronomy as his life's work, and in that year he became assistant in the Observatory at Lund. In 1888 he was appointed Professor of Astronomy and director of the Observatory of Upsala. This post he held for twenty-one years, until his retirement at the age of seventy. He died at Stockholm on 10 November 1914.

Dunér gave much attention to stellar spectroscopy, but the work for which he will be remembered was on the rotation of the Sun. It had been known for some time from observations on sun-spots that the Sun did not rotate as a whole, but few were prepared for the astonishing discovery which Dunér announced in 1891. Selecting two iron lines in the red portion of the solar spectrum, he compared their positions with a pair of oxygen lines, terrestrial in their origin and unaffected by the Sun's rotation. Dunér found a rotation period of 25½ days on either side of the equator. Then he measured

the rotation up to within 15 degrees of the poles, in regions where there are no sun-spots, and whose rotation period had never been measured. He found the period in these regions to be 38½ days. 'I must confess', he wrote, 'that this difference between the rotation period in the different latitudes appears to me incomprehensible and constitutes one of the most difficult problems in astrophysics.'

Space forbids mention of other early pioneers of astrophysics; but reference must be made to the great American astronomer who did more than any other man to co-ordinate and correlate the known facts of astrophysics. **Edward Charles Pickering** was born at Boston, Mass., on 19 July 1846, of an old New England family. After graduating at Harvard at the age of nineteen, he became Professor of Physics at the Massachusetts Institute of Technology. His interest in astronomy led to his inclusion in two eclipse expeditions in 1869 and 1870. In 1876, at the age of thirty, he was appointed to the important post of Professor of Astronomy at Harvard, and at once entered on his long and active career as an astronomer.

His first work was in photometry - the determination of the exact magnitudes of the stars, a field of research then somewhat neglected. With the aid of the 'meridian photometer' invented by himself, he determined in the three years 1879 to 1882 the exact brightness to within a fraction of a magnitude of 4,260 stars. Later the survey was extended to the southern hemisphere. His interest in stellar brightness led him to make a close study of variable stars, and in 1880 he proposed his famous classification of these objects. The star Algol occupied much of his attention, and he was the first to suggest that spectroscopic observations of the star's radial motion might decide for or against the eclipse theory. It fell to Vogel to carry through the crucial experiment and thus to detect the first spectroscopic binary star. Pickering, however, discovered two other spectroscopic doubles—

Mizar and Beta Aurigae—so that it may be said that he and Vogel between them share the credit for inaugurating this fruitful line of research.

Pickering's chief work, however, was his great spectroscopic classification of the stars. Henry Draper, a pioneer in stellar spectroscopy, died in 1882 at the age of forty-five. His widow, anxious that his work should be carried on, provided funds for the compilation of a catalogue of stellar spectra under Pickering's direction. The spectra of 10,351 stars were photographed at Harvard, and were catalogued in 1890 as the first *Henry Draper Catalogue*. The stellar spectra were classified more elaborately than ever before. Secchi's first type was divided into classes B and A, his second type into classes F, G, and K. The third type became class M, and the fourth class N, while the Wolf-Rayet stars and the nebulae became classes O and P respectively. Assuming the nebulae to be parents of the B stars, the Harvard sequence OBAFGKMN was assumed by Pickering and by the majority of his contemporaries to be the order of stellar evolution.

The Draper catalogue was a mere preliminary, however, and no sooner was it completed than Pickering set to work on a much larger catalogue which occupied nine volumes of the Harvard *Annals*. The spectra of no fewer than 225,000 stars were discussed in this great work. Pickering could not have carried through this work single-handed. He had a number of able assistants, chief among whom must be mentioned Mrs. Fleming—a Scotswoman—Miss Maury, and Miss Cannon. The catalogue was rendered of great value, inasmuch as it contained southern as well as northern stars. Pickering decided in 1890 to set up a southern branch of Harvard Observatory, and this was erected at Arequipa, on the slope of the Andes in Peru. His brother, W. H. Pickering, and his colleague, S. I. Bailey, were successively in charge of this station.

Another characteristic work of Pickering was his plan of

surveying the sky by means of photography. When presenting the Gold Medal of the Royal Astronomical Society to Pickering in 1901, the late H. H. Turner said that 'the energetic director of the Harvard Observatory . . . charts the sky once a month. . . . More than this, with a smaller instrument and on a smaller scale he charts the brighter stars every fine night. So that if a star brighter than the sixth magnitude appeared in any quarter of the heavens he would have a record of it on the first fine night.' Of this novel device for preventing any celestial changes from escaping notice, his colleague, Dr. Bailey, said:

'His conception of a vast collection of photographs of the stars, destined in time to give a history of the sky, was unique. Its execution was carried forward with zeal and success. These half-examined plates, made, in many cases, only for the purpose of securing as complete a record as possible, appeared to many as unnecessary and extravagant, and even excited ridicule. This seems absurd now that their value has been so fully demonstrated. Hardly a new star or variable has been discovered in recent years whose history could not be traced in a large degree upon these photographs.'

Pickering remained in harness to the end, and was active until shortly before his death on 3 February 1919.

We conclude with a reference to two brilliant American astrophysicists.

George Ellery Hale, our chief authority on things solar, was born in Chicago on 29 June 1868. He studied first at Harvard, and then at the University of Berlin; and on the completion of his course at the latter University, while still a very young man, he started experimenting on photography of the solar prominences, and shortly afterwards he began observations at his little private observatory at Kenwood, near Chicago. At the early age of twenty-four he took his place at a single stride among the leading astronomers by his invention of the spectroheliograph, by means of which it

has been found possible to photograph the Sun layer by layer and to analyse the various strata in the solar atmosphere. This instrument was independently devised by Henri Deslandres, then assistant to Janssen at Meudon, near Paris.

The principle of the spectroheliograph has been so well explained by Professor R. A. Sampson that we cannot do better than make use of his words:

In a spectogram, or photograph of a spectrum, 'each line is a record of the presence and the state of a separate chemical element at the spot of the disc to which the slit is directed. If this record could be read for that special line for the whole disc, we should have the same information summed up for the whole Sun. . . . Let the light from the line in question be allowed to pass to the photographic plate, by means of a second slit, at the focus of the camera, the jaws of which shut off all the rest of the spectrum. Let both the first and the second slits be long enough to extend right across the image of the Sun. Move the image of the Sun across the first slit, then the light which passes through the second slit will come at every moment from different strips of the Sun's surface: and if the photographic plate be moved behind the second slit, in unison with the movement of the Sun's image across the first slit, a record will be given, not of the radiations of every substance mixed together, as in ordinary photographs or visual observations of the Sun's disc, but of the states of some isolated substances as hydrogen or calcium, and even of different strata of these.'

At the early age of twenty-nine, Hale was selected as director of the new Yerkes Observatory at Williams Bay, Wisconsin, and in 1897 he entered on his duties there. Conjoined with this post was a professorship of Astrophysics in the University of Chicago. His residence at the Yerkes Observatory, however, was not to be of long duration. In the early years of the century the Carnegie Institution of Washington decided to erect on Mount Wilson in California what was known as a 'solar' observatory. Quite evidently there was one man supremely qualified for the task of organizing,

equipping, and afterwards directing a solar observatory—namely, the man who had become the leading authority on the Sun. Accordingly in 1905 Professor Hale accepted the post of director of the Mount Wilson Observatory, and during his eighteen years of office he surrounded himself with a group of the ablest astronomers who have ever worked together in collaboration. Mount Wilson became in a very real sense the centre of gravity of the astronomical world. Two great telescopes—the 60-inch and 100-inch reflectors—were placed at the disposal not only of the members of the staff but also of the many research associates—distinguished foreign astronomers—who paid periodic visits to the Observatory. Hale proved himself a great organizer, and when overstrain caused him to retire from the directorship in 1923, the Observatory was fortunate in retaining him as honorary director.

Despite his heavy administrative duties both at Yerkes and Mount Wilson, Hale continued his work with the spectroheliograph and went from discovery to discovery. In 1903 the development of the instrument permitted the photography not only of calcium but of hydrogen clouds. In 1908, at Mount Wilson, Hale photographed the solar disc in the red light of hydrogen. These photographs revealed whirling storms in a region of the solar atmosphere above the calcium and hydrogen clouds. This strongly supported the theory of the vortical nature of sun-spots. 'We know now', said Hale, 'that they are caused by vortices in the solar atmosphere and the various theories which do not recognize this fact may be laid aside.' Sun-spots now monopolized most of his attention. In 1909 he was able to announce a highly important discovery. In 1896 the Dutch physicist Zeeman had shown that light from a luminous vapour is altered in a certain way when under the influence of a strong magnetic field: the lines of the spectrum are widened or broken up into several constituents. This is called the Zeeman effect. Hale in 1908 closely

scrutinized the spectra of sun-spots for traces of the Zeeman effect, and he was soon rewarded by detecting double and triple lines indicating clearly the existence of a magnetic field in sun-spots. This discovery registered a long step forward in our knowledge of the Sun.

In 1912 Hale outlined what he called a 'tentative working hypothesis' as 'a guide to further research'. According to this theory, a column of gas moves upward from the interior of the Sun towards the surface of the photosphere. Owing to differences of velocity of adjoining surfaces or irregularity of structure, a vortex motion is set up. The circulation in the vortex is vertically upward and then outward. As a result of expansion in the central portion of the vortex, cooling sets in, and a comparatively dark cloud—the sun-spot umbra—is formed. 'A rapid flow of negative ions sets in towards the cooler gases at the centre from the hotter gases without. These ions, whirled in the vortex, produce a magnetic field.'

Perhaps the most striking of Hale's subsequent discoveries was that of invisible sun-spots, announced in 1922. Most sun-spots, as Hale pointed out, 'are associated in pairs, of opposite magnetic polarity'. He concluded, therefore, that single spots are single only in appearance, the visible spot being associated with an invisible spot 'in which the cooling due to expansion is insufficient to cause perceptible darkening of the Sun's surface'. It occurred to him that it might be possible to pick up these invisible spots by means of the Zeeman effect, and a systematic search was rewarded by the discovery of two invisible spots in November 1921.

Henry Norris Russell was born at Oyster Bay in the state of New York on 25 October 1877. Educated at Princeton University, New Jersey, he was initiated into astronomy by the famous Charles Augustus Young. In 1902 he proceeded to England and studied at Cambridge, doing practical work at the Observatory there under Sir Robert Ball. After his return to America he succeeded Young in the Princeton

Observatory. He has worked extensively at Mount Wilson, and from time to time has been one of that Observatory's research associates.

Russell will go down to fame as the co-discoverer of the division of the stars into the two main classes of giants and dwarfs, and the author of the most comprehensive theory of stellar evolution ever promulgated. Both of these achievements were by-products of his intensive work on stellar parallaxes, carried on in England as well as in America.

As early as 1905 Hertzsprung of Potsdam had pointed out the existence of two well-defined classes of stars which he called 'giants' and 'dwarfs'. Eight years later Russell, as the upshot of his parallax measures, adduced confirmatory evidence so strong that the separation of the stars into these two classes has never since been called in question.

'There are', he wrote in December 1913, 'two great classes of stars—the one of great brightness, averaging perhaps a hundred times as bright as the Sun, and varying little in brightness from one class of spectrum to another: the other of smaller brightness, which falls off very rapidly with increasing redness. . . . The two groups, on account of the considerable internal differences in each, are only distinctly separated among the stars of class K or redder. In class F they are partially, and in class A thoroughly intermingled, while the stars of class B may be regarded equally well as belonging to either series.'

At this time the Vogel-Pickering evolutionary scheme was all but universally accepted by astronomers: the Harvard sequence was supposed to represent the order of stellar development. It is true that Lockyer had suggested a different order from red stars through yellow and white stars and back again to red, but the consensus of opinion was against this view.

Doubt was cast in Russell's mind on the generally accepted order of evolution by the discovery that some stars of Secchi's first type had a greater density than those of the second.

'The order of increasing density', said Russell, 'is the order of advancing evolution. . . . The giant stars then represent successive changes in the heating up of a body and must be more primitive the redder they are: the dwarf stars represent successive stages in its later cooling, and the redder of these are the furthest advanced. We have no longer two separate series to deal with, but a single one, beginning and ending with class M and with class B in the middle—all the intervening classes being represented in inverse order in each half of the sequence.'

Russell's theory showed that as the giant stars grow hotter they contract. This goes on until a certain critical stage is reached, when the star becomes too dense to obey the laws of a perfect gas: then the temperature begins to fall and the star's brightness decreases rapidly. The theory seemed to explain practically everything except the way in which stars are generated from nebulae; and one by one the leading astronomers accepted it. Then in 1924 Eddington showed by a rigorous mathematical investigation that the assumption that the dwarf stars did not obey the perfect gas law was an unsound assumption; for the fact of ionization had been neglected in previous investigations. It is evident that if the dwarf stars obey the perfect gas laws, the theoretical basis of Russell's theory disappears. On the basis of certain suggestions of Eddington, however, Russell revised his original theory in 1925. The revised scheme is not very unlike the original, and Russell has claimed that it is more comprehensive. It is impossible to forecast the future development of cosmogony; but it may be affirmed that at the present time Russell's revised scheme of stellar evolution holds the field.

VII

WATCHERS OF THE SKIES

JOHANN FRIEDRICH JULIUS SCHMIDT—EDUARD SCHÖNFELD—GIOVANNI VIRGINIO SCHIAPARELLI—CAMILLE FLAMMARION—PERCIVAL LOWELL—WILLIAM HENRY PICKERING—EDWARD EMERSON BARNARD—MAX WOLF

WITH the application of the spectroscope to astronomy, as the last chapter has shown, a new type of astronomer arose. Immediately after Kirchhoff interpreted the hieroglyphics of the solar spectrum, a considerable number of astronomers chose to concentrate on the new astronomy. Several others, however, either for lack of the necessary equipment or by definite choice, continued to tread the older paths and explore the more familiar fields. These true 'watchers of the skies' included some of the most distinguished astronomers of all nationalities of the later nineteenth and earlier twentieth centuries.

We begin with a great German whose work on the Moon secured him a lasting place among the famous astronomers. **Johann Friedrich Julius Schmidt** was born at Eutin, in Lübeck, on 25 October 1825. Like so many distinguished astronomers, he was devoted to astronomy from his boyhood. A copy of Schröter's book on the Moon came into his hands when he was fourteen; this fixed his choice. His father possessed a small telescope, little better than a toy. Schmidt fastened it to a lamp-post and began with boyish enthusiasm to study the Moon nightly. But his study was to some purpose: he was no mere 'Moon-gazer'. He sketched the more prominent craters and re-examined all the objects noted by Schröter. He had already a good deal of work to his credit when at seventeen he entered the High School at Hamburg. Thence he went to Altona Observatory as an assistant and while employed there a copy of Beer and Mädler's great chart

of the Moon came into his possession. In 1845 he went to the Bilk Observatory near Bonn, where he studied meteors, and a year later he was called to Bonn as assistant to Argelander, with whom he was associated in the spade-work for the *Durchmusterung*.

In 1853 Schmidt went to Olmütz as director of the Observatory there, and five years later he was invited by the Greek Government to go to Athens as director of the National Observatory there. Here he remained for the rest of his life. Favoured by the clear Greek skies, he made many important discoveries, among them the new star of 1876. The Moon, however, remained his first love and chief object of study. Notwithstanding his many changes of location, his movement from one observatory to another, he carried on his lunar work with steady perseverance, amassing the many observations necessary for the great chart which he had projected in his youth. By the year 1868 the chart, in twenty-five sections, delineating 32,856 craters, was practically completed, and by 1874 it had been finally revised—thirty-four years after he had determined, as a boy of fourteen, to undertake the work. Four years later the chart was published at Leipzig at the expense of the German Government. It was said by an Irish astronomer, John Birmingham, that the completion of this map by a single observer

'must seem almost incomprehensible to a man of ordinary powers. . . . We have first the astronomer as a youth of fourteen, viewing the Moon with a little telescope steadied by a lamp-post and probably the laughing-stock of many a passer-by: afterwards he is found in his maturer years, pursuing his favourite study under more or less indifferent circumstances, until at length as director of a national Observatory he completes the wonderful production of his truly inimitable labours. For this it required the unswerving persistence that is ever a chief attribute of genius.'

In the course of his long-continued study of the Moon Schmidt was led to the conclusion that Mädler had been

somewhat over-confident as to the absence of change of any kind. He announced in October 1866 that the small crater Linné, on the floor of the Mare Serenitatis, described by Mädler as a deep crater five or six miles in diameter, had been obliterated: or rather, had been transformed into a whitish spot, with a small pit in the centre. Controversy raged as to the actuality of the change which Schmidt believed himself to have detected. But his skill as an observer was recognized even by his critics; and the balance of evidence has long since been recognized as in favour of a real change, volcanic or otherwise, which disposes of the idea that the Moon is completely without change. Schmidt died suddenly at Athens on 8 February 1884.

Eduard Schönfeld was born at Hildburghausen, in Meiningen, on 22 December 1828. He received his early education at home from his mother, and was able when he went to school to outstrip all the boys of his own age and to act as a kind of unofficial tutor to his contemporaries. While still at school he began to study astronomy, and felt a strong inclination to adopt it as his life-work. His father, however, objected on the ground that it had little to offer in the way of remuneration, and young Schönfeld went to Hanover and then to Cassel to train as an architect. In 1849 he enrolled at Marburg University to study physics under Bunsen, and in this way his old interest in astronomy was revived. In 1851 he paid a visit to Bonn and succeeded in being received there by Argelander, then in the heyday of his fame. What one of Schönfeld's biographers called 'the charm of Argelander's personality' so impressed the young man that he resolved to study at Bonn and to decide after all for an astronomical career. If Schönfeld was attracted by Argelander, the older man was equally attracted by the younger. He not only received Schönfeld as a student with open arms, but appointed him to an assistantship before he took his degree in 1854.

Schönfeld was twenty-six when he graduated, and he plunged right into his life-work. At this time Argelander was in the thick of his observational work for the *Bonn Durchmusterung*, and for the next seven years Schönfeld was his invaluable understudy. Indeed a great deal of the credit for the great undertaking must be given to Schonfeld—a fact which Argelander would have been the last to dispute. Through the influence of his chief, Schönfeld was appointed director of the Observatory at Mannheim in 1859. Argelander deeply regretted losing him from Bonn, but was anxious for his assistant's advancement. It is doubtful, however, if the change to Mannheim was really beneficial. The Observatory was badly equipped, and although Schönfeld accomplished a good deal of work on nebulae and variable stars, he had not the wide opportunity for research which he had enjoyed at Bonn. His exile from Bonn, however, was but temporary. Argelander died in February 1875, and the natural and obvious successor was his friend Schönfeld, who accepted the appointment; and it was at Bonn that the remainder of his busy life was to be spent.

The work for which he will be remembered with gratitude was his extension of the *Durchmusterung*. Argelander's great survey was incomplete: it included all the stars in the northern celestial hemisphere and those down to 2 degrees south of the equator, but there it stopped. Schönfeld determined to take in as much of the southern celestial hemisphere as could be conveniently surveyed from a northern observatory. He used a more powerful telescope with smaller fields, and between 1875 and 1884 626 zones were surveyed. The observations made in these years were the basis of the extended *Durchmusterung*, containing 133,659 stars.

All this work was personally carried through by Schönfeld, and he overtaxed his strength in the accomplishment of his task. It was not unusual for him to sit down at his desk at nine in the morning and work steadily on at desk and tele-

scope until three o'clock in the following morning. There can be little doubt that this stern application to duty undermined his constitution and hastened his death; and he died at Bonn on 1 May 1891, in his sixty-third year. But he had accomplished a great task, and had carried to a successful completion the work of his master Argelander. Between them, Argelander and Schönfeld had surveyed and catalogued 457,857 stars.

Giovanni Virginio Schiaparelli, perhaps the greatest 'watcher of the skies' whom the later nineteenth century produced, was born at Savigliano, in Piedmont, on 14 March 1835. After receiving an elementary education in the high schools of his native town, he proceeded, at the early age of fifteen, to the University of Turin, where in 1854 he graduated with honours. At the University he specialized in mathematics, architecture, and engineering, and was seemingly destined for an eminently practical and presumably lucrative career. After graduation, however, he changed his plans. 'Without taking into account my almost absolute poverty,' he stated in a letter written in old age,[1] 'I declined entering into either of these lines, and formed the project of devoting myself to astronomy, which was not done without much opposition on the part of my parents.' For a short time he taught mathematics in one of the Turin schools, but this appointment served merely as a stop-gap. Meanwhile his brilliant gifts had attracted the attention of several influential people, who prevailed upon the Government of Sardinia and Piedmont to make him a small annual grant for a few years, to enable him to go abroad to pursue his studies. Accordingly he went to Berlin, where Encke was at the height of his fame, and studied for two and a half years under that great master. Thence he proceeded to Pulkova, where under the direction of the Struves he trained himself as an observer.

While Schiaparelli was abroad great changes took place in his native land. The Kingdom of Italy came into existence,

[1] To the present writer, in 1903.

and one of the first acts of the new Government was to appoint an assistant astronomer at the Brera Observatory in Milan. The Government chose Schiaparelli, who returned from Pulkova to take up his new duties, on which he entered in July 1860. Nine months later he discovered an asteroid, Hesperia, number sixty-nine of the asteroid family. In pre-photographic days the discovery of a new asteroid attracted attention, and so Schiaparelli became famous; and on the death of his chief Carlini in September 1862 he was appointed director of the Observatory, a post which he held for thirty-eight years.

In 1866 Schiaparelli announced his first great discovery, the connexion between comets and meteors. The subject of meteors was 'in the air' at the time, and several of the ablest astronomers were occupying themselves with it. Towards the end of 1866 Schiaparelli, in four letters addressed to Secchi, recapitulated the assured facts about meteors. These bodies, he showed, were members of the Solar System moving with greater velocity than the Earth along tracks resembling those of comets in eccentricity and inclination. Further, he announced that, in one case at least, a meteor track was identical with a cometary orbit. Having computed the path of the Perseid meteors, he found it to be the same as that of the comet of 1862. A few months later he showed, independently of Le Verrier, who had also been working on the subject, that the Leonid swarm moved in the same orbit as that of Tempel's comet.

These investigations set Schiaparelli's keen mind thinking on the real nature of comets. A comet he defined as a cosmical cloud formed in space by 'the local concentration of celestial matter'. In his notable book, *Le Stelle Cadenti*, based on three lectures delivered before the Royal Institute of Lombardy, he wrote: 'The meteoric currents are the products of the dissolution of comets and consist of minute particles which certain comets have abandoned along their

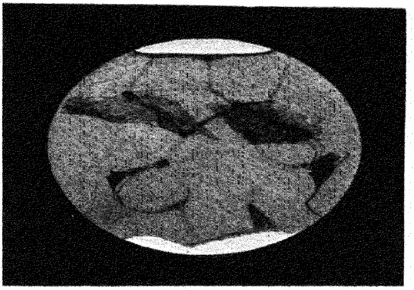
orbits by reason of the disintegrating force which the Sun and planets exert on the rare material of which they are composed.' Comets are gradually elongated, cease to shine as comets, and are transformed into meteor streams. Schiaparelli's successful solution of the comet-meteor problem was rewarded by the gift of the Gold Medal of the Royal Astronomical Society in 1872.

Schiaparelli, however, will be chiefly remembered as an observer of the planets, and more particularly of the Earth's fellow dwarfs—Mars, Venus, and Mercury. His work on Mars was indeed epoch-making. Percival Lowell, his famous disciple, called him 'the Columbus of a new planetary world', and the tribute was not undeserved. In August and September of 1877 Mars was most favourably placed for observation; and Schiaparelli commenced his long-continued work. In September, while carrying out a trigonometrical survey of the disc, he noticed that the reddish-ochre portions of the planet — the 'continents' of the earlier maps—were cut up by numerous dark streaks, which he named *canali*, an Italian word which may be rendered as 'canals' but more correctly as 'channels'. It subsequently appeared that Beer and Mädler and later Dawes and Secchi had seen some of these markings, but had simply classified them as 'straits'; but it was Schiaparelli who first recognized them as distinctive features of the planet. In 1879 he again observed the canals and noticed to his great surprise that one of them had become double. At the opposition of 1882–3 he noted the gemination, as he called it, of several other canals. 'The observation of the gemination', he wrote, 'is one of the greatest difficulty and can only be made by an eye well practised in such work, added to a telescope of accurate construction and of great power.'

These discoveries of Schiaparelli were made in the face of a great deal of scepticism. Perhaps Lowell overshot the mark when he said that 'the world was anything but prepared for the revelation, and when he announced what he had seen

promptly proceeded to disbelieve him'. Yet the fact remains that there was a disposition on the part of quite a number of astronomers to doubt the reality of his discovery, despite his high reputation as an accurate observer. For nine years he was the only astronomer who was able to see the canals, and this of course made for general scepticism. In 1886, however, Perrotin and Thollon, the French astronomers, using the great telescope of the Nice Observatory, confirmed Schiaparelli's observations, and later the canals were seen by Flammarion in France and by several eminent American astronomers, chief among them Lowell and W. H. Pickering.

In 1892 failing sight compelled Schiaparelli to bring his long-continued investigations of Mars to an end. His fifteen years' study led him to conclude that 'the climate of Mars must resemble that of a clear day upon a high mountain. By day a very strong solar radiation, hardly mitigated at all by mist or vapour, by night a copious radiation of the soil towards celestial space and because of that a very marked refrigeration.' At the close of his series of observations, he acquiesced in the current opinion that the blue-green areas were seas, although his own observations had thrown some doubt on this assumption. He spoke of the network of canals as 'perhaps constituting the principal mechanism (if not the only one) by which water (and with it organic life) may be diffused over the arid surface of the planet'. The canals he regarded as waterways lined on either side by banks of vegetation, but he was mystified as to their origin, and still more so as to their gemination. He would not dismiss lightly the suggestion that the canal-system was the handiwork of intelligent beings. 'I am very careful not to combat this supposition, which includes nothing impossible.' At a later stage he inclined more and more to the intelligence theory as advanced and developed by Lowell. In 1897 he remarked that the system 'presents an indescribable simplicity and symmetry which cannot possibly be the work



MARS
From a drawing by Lowell

COMET MOREHOUSE 1908
From a photograph by Max Wolf

of chance'; and in 1905 he wrote to Lowell, 'Your theory of vegetation becomes more and more probable.'

Schiaparelli's observations on Mercury and Venus were carried on simultaneously with his Martian studies. The chief aim of these observations was to settle the vexed question of the rotation periods of the two interior planets. Both Venus and Mercury, as is well known, are very difficult to observe, and neither can be long kept under observation after sunset or before sunrise on account of proximity to the Sun. Schiaparelli decided on a new method of observation. He observed both Mercury and Venus in daylight, believing rightly that the disadvantages of daylight study were more than counterbalanced by the advantage of keeping the planets under observation for long periods at a time. The observations on Mercury were the first to be completed. In December 1889 Schiaparelli announced that Mercury performs only one rotation on its axis during its revolution round the Sun, that its day and year are of equal length. One hemisphere of the planet enjoys perpetual day while the other is in everlasting night. He pointed out, however, that owing to the planet's libration, resulting from uniform axial motion and irregular orbital motion, the Sun rises and sets on about one-fourth of the planet's surface. In the following year he announced that Venus rotated in a similar manner—its period being 225 days, equal to its year in length. A second series of observations in 1895 confirmed the first. In the case of Mercury, Schiaparelli's conclusions have been generally accepted, and they find a theoretical explanation in the tidal friction theory. Just as the Earth has slowed down the Moon's rotation, so the Sun has slowed down Mercury's. In regard to Venus there has been much divergence of opinion. Evidence has been conflicting, and at the present time the question may be said to be still an open one.

Schiaparelli's attention to double stars was constant. Between 1875 and 1899 he had made over 11,000 measures;

and his work on the distribution of the stars constituted an important contribution to the problem of the structure of the Universe. He constructed a series of planispheres, giving the star-density in every part of the heavens for stars of different magnitudes, and confirmed the truth of Proctor's contention that the stars visible to the naked eye tend to aggregate on the Milky Way. All unwittingly, Schiaparelli and other astronomers who reached similar conclusions had actually discovered the 'local cluster' to which our Sun and the nearby star belong.

Schiaparelli's work on the history of astronomy was of a high order, and his papers on Greek and medieval astronomical systems are recognized as authoritative. So, too, was his little book on *Astronomy in the Old Testament*, translated into English in 1905. His close acquaintance with Hebrew and other oriental languages stood him in good stead in these researches. He was indeed a linguist as well as a scientist, and a theologian as well. His assistant and successor, Celoria, indeed, went so far as to say that 'there have been and are few men in Italy so competent as Schiaparelli to occupy a chair of comparative religion'. His was a many-sided mind, and his intellectual activity was ceaseless.

Failure of eyesight compelled Schiaparelli in 1900 to retire from the directorship of the Brera Observatory, over which he had for so long presided and on which he had conferred undying fame. His years of retirement were by no means a time of inactivity, and despite almost total blindness he was fully occupied with his scientific studies. In his seventy-sixth year he suffered an attack of apoplexy, and died without much suffering on 4 July 1910.

Camille Flammarion, whose popular fame as a writer and lecturer on astronomy somewhat obscured his ability as an astronomer, was born at Montigny-le-Roi in the department of Haute Marne, on 25 February 1842. His parents intended him for the priesthood, and he received his early education

in the ecclesiastical seminary of Langres. From childhood upwards, however, he was fascinated by 'the starry sky which lights up' when 'earth falls asleep', and he had resolved, while still a mere lad, to become an astronomer. In 1858, at the age of sixteen, he entered the Paris Observatory as junior assistant to the famous Le Verrier. The post of assistant to the co-discover of Neptune was, however, no bed of roses. Le Verrier's life, as Flammarion said long afterwards, 'would have been still more useful to science and humanity if he had possessed a more sociable character and a more disinterested love for the general progress'. Many members of Le Verrier's staff could not tolerate the chief's autocratic methods and irascible temper, and Flammarion was no exception. In 1862, therefore, he left the Observatory, and for the next four years, while holding a post on the Bureau des Longitudes, took a course at the Sorbonne. Meanwhile he had blossomed forth as a writer on astronomy. His first book, *The Plurality of Inhabited Worlds*, was published in 1862.

In 1883 Flammarion established his private observatory —L'Observatoire Flammarion—at Juvisy-sur-Orge in the department of Seine-et-Oise. For some years he devoted himself to work on the Moon and the planets and his work on Mars and Venus was of permanent value. In 1876 he constructed a chart of the former planet based on the drawings of various observers: this Lowell considered worthy to rank among the historic maps of Mars. In 1892 Flammarion published his book *La Planète Mars*, which unfortunately was not translated into English. This volume was described by an English author as 'the standard work on Mars for many a year to come'. It was, however, soon superseded by the works of Lowell.

On the question of the physical condition of Mars, Flammarion always held to what we may call the evidence of the senses, namely, that the temperature of the planet is much higher than those who calculated it on purely theoretical

grounds would admit. He was one of the earliest observers of the Martian canals after their discovery by Schiaparelli, and while not definitely committing himself to the Lowellian interpretation of their nature, he stoutly maintained their objective reality. Messrs. Evans and Maunder in 1902 made their famous experiment with the boys of Greenwich Hospital School; and the fact that the boys drew lines between numerous dots on pictures of the planet was hailed as a great triumph for the 'optical illusion' theory. Flammarion, however, destroyed the evidential value of the experiment by repeating it with French schoolboys, who drew no lines at all. Flammarion held that Mars was not only habitable but inhabited. 'As to the inhabitants of Mars,' he wrote about twenty years ago, 'this world is in a situation as favourable as the Earth for habitation, and it would be difficult to discover any reason for perpetual sterility there. It appears to us on the contrary to be a very living world.'

Flammarion's long-continued study of Venus with the Juvisy refractor did not bear out the conclusion of Schiaparelli as to the period of rotation. His own observations harmonized with a rotation period of about twenty-four hours, and in a letter to the writer in 1921 he reiterated his belief in a short rotation period. His work on the Moon convinced him long before the later work of Elger and W. H. Pickering that the changes in tint could not be explained by varying conditions of illumination. 'Geological and even meteorological changes', he wrote in 1879, 'seem still to be at work on the surface of our satellite.' The arena of the walled-plain Plato, he pointed out,

'darkens as the Sun illuminates it more, which seems opposed to all imaginable optical effects. . . . The odds are 99 to 1 that it is not the light which produces this effect, and that it is the solar heat which we do not sufficiently take into account when we are considering the modification of tints observed on the Moon, although it may be quite as intimately connected as the light with

the action of the Sun. It is highly probable that this periodical change of tint on the circular plain of Plato, visible every month to any attentive observer, is due to a modification of a vegetable nature caused by the temperature. . . . Far, then, from having a right to assert that the lunar globe is destitute of any vegetable life, we have facts of observation which are difficult, not to say impossible, to explain if we assume a soil purely mineral and which on the contrary are easily explained by admitting a vegetable coating of whatever form it may be.'

This was written in 1879, and Flammarion was then a voice crying in the wilderness. Here again his work was in the nature of what may be called scout work, and had he concentrated on lunar astronomy, he might have largely anticipated the work of W. H. Pickering. Flammarion's last important piece of work was his revision of Messier's catalogue of clusters and nebulae. His activity was prodigious, and to the very end of his long life he was in harness. He died at Juvisy-sur-Orge on 4 June 1925, in his eighty-fourth year.

Percival Lowell, one of the best-known astronomers of the past generation, was born in Boston, Mass., on 13 March 1855. He came of a distinguished New England family: his father, Augustus Lowell, was a cousin of James Russell Lowell, the poet. After a thorough preliminary education Percival Lowell entered Harvard College, where he graduated in 1876. Even at this early stage he was a many-sided man, equally devoted to history and to science. Possessed of means and leisure, he travelled extensively in Japan and Korea, residing for several years in the former country. Four of his earlier volumes dealt with the Far East and its peoples.

Lowell's interest in astronomy dated from 1870, when as a boy of fifteen he was fascinated by the planet Mars. But his many-sidedness and his interest in so many subjects prevented him from settling down to a specialized course of study, and it was not until 1894, when he was thirty-nine

years of age, that he commenced systematic astronomical work. His early interest in astronomy was reawakened by the controversy regarding Schiaparelli's discovery of the canals of Mars. Accordingly he decided to devote the remainder of his life to astronomy and to erect an observatory for the special study of the surface-markings of the planets, and more especially of Mars. 'A steady atmosphere', he pointed out in 1895, 'is essential to the study of planetary detail—size of instrument being a very secondary matter. A large instrument in poor air will not begin to show what a smaller one in good air will. When this is recognized, as it eventually will be, it will become the fashion to put up observatories where they may see rather than be seen.' After testing the atmospheric conditions of a number of widely separated localities, Lowell fixed on Arizona, and at Flagstaff, at an elevation of over 7,000 feet, he established early in 1894 the Lowell Observatory, one of the most famous astronomical institutions in the world. The Observatory was temporarily transferred in the winter of 1896–7 to Tacubaya near the city of Mexico, but the experiment was not repeated. Lowell's subsequent life was spent either at the Lowell Observatory or, when there was less opportunity for planetary work, at his home in Boston. In 1902 he was appointed non-resident Professor of Astronomy at the Massachusetts Institute of Technology.

The study of Mars was commenced on 24 May 1894 with the aid of an 18-inch refractor by Brashear, replaced two years later by a 24-inch by Alvan Clark. In 1895 Lowell published his first book on *Mars*, in which he summarized the results of his observations and those of his assistant, A. E. Douglass, and his friend, W. H. Pickering. For the first time in the history of Martian discovery the polar cap was observed to disappear. In addition Lowell discovered a large number of new canals and lakes, which he renamed as 'oases'; while Douglass detected the presence of canals in the

dark regions of the planet—the so-called 'seas'—thus finally refuting the theory of their aqueous nature.

In this book too Lowell put forward his hypothesis of life on Mars, and in his later volumes, *Mars and its Canals* and *Mars as the Abode of Life*, it was further developed. He contended that the canals were strips of fertilized ground on either side of waterways constructed by the Martians to convey water from the melting snowfields to the parched equatorial regions. Perhaps the startling nature of the theory, and the manner in which so many astronomers dismissed it as incredible, somewhat detracted from Lowell's standing in the astronomical world. Nevertheless, he persevered in his researches, collecting a vast amount of information, all of which tended to confirm his theory. In 1903 his study of the cartouches, a series of curves representing the visibility of the canals, led him to the opinion that 'the behaviour of the canals in action leads to the same view of their nature as their appearance at rest'. In 1905 he and his assistants succeeded in photographing the canals, and he hailed his success as a further confirmation of his theory. 'The camera', he said in a letter to the writer, 'does not agree with the armchair critics of the canals, but will have it that these markings are lines.' His photographic success, repeated at subsequent oppositions, if it did not make many converts to his theory, was at least a severe blow to the hypothesis that the canals were due to optical illusion.

Even opponents of his theory latterly regarded him as the chief authority on Mars after Schiaparelli's death. At each opposition Lowell announced some new fact or facts hitherto undetected. 'Each opposition as it comes round', he said, 'adds something to what we knew before. It adds without subtracting.' In an article published a short time before his death, Lowell claimed that, 'since the theory of intelligent life on the planet was first enunciated 21 years ago, every new fact discovered has been found to be accordant with it. Not a single thing has been detected which it does not explain.'

Lowell's observations of Mercury and Venus were commenced in 1896. Like Schiaparelli, he observed these planets in daylight, and he reached conclusions in harmony with the great Italian astronomer—that both planets rotated on their axes in the same time that they revolved round the Sun. On the surface of Mercury he detected no trace of atmosphere. He noted 'narrow, irregular lines, very dark. . . . "Cracks" best explains their appearance, and probably their nature.' Over the observations of Venus controversy raged for a considerable time. The long rotation period—equal to the period of revolution—found by Schiaparelli was confirmed. But in 1900 Bélopolsky, by the spectrographic method, found a short period. In 1903 Lowell, aided by Dr. Slipher, who had by then become his assistant, made a series of spectrographic observations which confirmed the long period. 'The evidence of the spectroscope', Lowell wrote, 'is against rotation of short duration, and, so far as its measure of precision admits, the investigation confirms a rotation period of 225 days.'

But it may be said that Lowell's greatest discovery was a posthumous one. A year before his sudden and unexpected death on 12 November 1916, he published his *Memoir on a Trans-Neptunian Planet*, in which he sought to locate the position of a member of the Solar System more distant than the outermost of the giant planets. Other astronomers before Lowell had suspected the existence of such a body, and had attempted to find it, but their investigations had led to no result. This was not to be wondered at, for the data were very slender. Neptune has not, even yet, betrayed any sign of the pull of an unknown planet, but astronomers have been aware for years past of minute 'residual irregularities' in the motion of Uranus which cannot be explained by the action of Neptune. Lowell made these irregularities the starting-point of his search, and he found for the supposed planet a distance of over 3,720 millions of miles from the Sun, a

period of a little under 300 years, and a mass of seven or eigh times that of the Earth. He likewise deduced that the plane would be found in Gemini or in a region of the sky exactl opposite, in Sagittarius; but he was unable to decide betweei these regions. He believed that the planet would be equa in brightness to a star of the twelfth or thirteenth magnitude but as both Gemini and Sagittarius are rich regions of th sky, crowded with faint stars, it was evident that the task o finding the trans-Neptunian planet would be very difficult.

Nevertheless, the astronomers of the Lowell Observator undertook an extensive search for the hypothetical planet and this search was systematically continued after Lowell's death. At length, early in 1930, the perseverance of th Flagstaff astronomers was rewarded, when a faint movin star-like point was found on several photographs of th constellation Gemini, and on 13 March 1930 Dr. Slipher announced that Lowell's planet had been found. The planet however, proved to be fainter and less massive than Lowel had supposed. Lowell believed he was locating a giant, ar exterior member of the outer group, akin to Uranus and Neptune; but Pluto, as the new planet has been called, has proved to be a dwarf, about 4,000 miles in diameter, smaller and less massive than Mars. It may be that Pluto is th nearest of a group of dwarf planets analogous to our Earth and its three near neighbours. At all events, Lowell has th same right to be called the discoverer of Pluto as have Adams and Le Verrier to be regarded as the discoverers of Neptune

William Henry Pickering, the younger brother of th more famous Edward Charles Pickering, was born in Boston Mass., on 15 February 1858. He graduated from the Massachusetts Institute of Technology in 1879; and in student days, inspired doubtless by the example of his brilliant brother, he paid some attention to astronomy. After some years, during which he hesitated as to a career, he was appointed in 1887 as assistant to his brother at Harvard

Observatory. Three years later he became assistant Professor of Astronomy in Harvard College.

E. C. Pickering had for some time contemplated the establishment of a Harvard auxiliary station, and in 1887 he sent his brother to Colorado to test the climatic conditions there. In 1888 W. H. Pickering went to California and erected a temporary observatory on Mount Wilson, where he remained for over a year. Here he secured his famous photograph of the Orion nebula which revealed almost the whole constellation of Orion to be wrapped in nebulous haze.

The Harvard authorities, however, decided to go farther afield than California. Arequipa, on the slope of the Andes in Peru, was finally selected, and for two years W. H. Pickering was in charge of the Harvard auxiliary station there. Here his effective career as an astronomer may be said to have commenced. In 1892 Mars was very favourably placed for observation, especially in southern latitudes. Schiaparelli had practically given up observing the planet owing to failure of eyesight, and the Lowell Observatory had not yet been erected; so at this opposition the chief work on Mars was done by W. H. Pickering. Favoured with the clear skies of Arequipa as well as the proximity of the planet, Pickering and his assistant, Mr. A. E. Douglass, made two discoveries of the highest importance. The first was that of the 'lakes', or as Lowell afterwards termed them, the 'oases' of Mars. 'Scattered over the surface of the planet,' Pickering remarked, 'chiefly on the side opposite to the two seas, we have found a large number of minute black points. They occur almost without exception at the junction of the canals with one another, and with the shaded portions of the planet.' The name 'lakes' was given to these objects by Pickering, in keeping with the hitherto accepted view that the shaded areas on Mars were oceans. Schiaparelli's observations, however, had thrown grave doubts on this view, and the second sensational discovery made at Arequipa finally discredited it.

Pickering noticed 'certain curved branching lines' in the dark areas. 'Some very well-developed canals cross the oceans. If these are really water-canals and water-oceans, there would appear to be some incongruity here.' But Pickering added, 'I very much doubt if what are usually known as canals and oceans contain any water at all.'

In 1893 Pickering returned to the United States, and for the next two years he worked in close collaboration with Percival Lowell. He assisted Lowell in the erection of the 18-inch telescope of the Lowell Observatory, and during the memorable opposition of Mars in 1894 he co-operated with Lowell in observation of the planet. The polar sea, observed by Lowell, was carefully studied by Pickering, whose observations by means of the polariscope showed it to be a genuine sea, although a temporary one. For the next few years Pickering was resident chiefly at Harvard. In 1898 he announced the discovery of a ninth satellite of Saturn. This was no merely accidental discovery: it was the outcome of a search carried on intermittently by means of photography for ten years. He found images of a moving object on a plate of Saturn's vicinity. He announced this as a new Saturnian satellite, 8 million miles from the planet, and he gave it the name of Phoebe. However, the new satellite could not be identified telescopically, and was not again photographed, and widespread scepticism began to manifest itself. Pickering was vindicated, however, in 1904, when he adduced overwhelming evidence for the new satellite's existence; and he greatly astonished the scientific world when he further announced that Phoebe revolved round Saturn in a retrograde direction and that its distance from Saturn fluctuated between 6 million and nearly 10 million miles.

When Pickering was working at Arequipa, some observations of the Moon caused him to doubt the accuracy of the conventional view of our satellite—namely, that its surface is, in the literal sense of the word, changeless, airless, and lifeless.

He resolved to embark on a course of intensive lunar observations, and in 1900, for the purpose of getting as good air as possible for observing and photographing the finer detail on the Moon's surface, he erected what was intended to be a temporary station near Mandeville, Jamaica. As a matter of fact, it developed into yet another Harvard auxiliary station and latterly became Pickering's private observatory and his home also. In the first eight months of the year 1901 Pickering and his assistants secured eighty plates, the basis of his monumental work entitled *The Moon*, published in 1903 at New York. In this book Pickering brought forward evidence to show that the Moon is not so dead as many people have believed. Fixing his attention on certain white spots which fluctuated in size, he found that their apparent diameter depended upon the lunar 'season', increasing during the long lunar night and shrinking in the daytime. 'The phenomenon', suggested Pickering, 'is evidently analogous to that of the changing size of the polar caps of Mars and of the Earth.' Grey spots also attracted his attention, and their fluctuation in size and in tint convinced him of the existence of something resembling vegetation on the Moon, 'coming up, flourishing, and dying, just as vegetation springs and withers on the Earth'. Since 1903 Pickering has collected a great deal of further evidence of a confirmatory kind. Nevertheless many if not most astronomers have been somewhat hesitant in accepting Pickering's conclusion. But it should be borne in mind that Pickering is the one prominent observer who under the most favourable atmospheric conditions has made a special study of the Moon, and that his views must be admitted to carry a great deal more weight than those of his critics who have not devoted any special attention to lunar astronomy.

The planets Mars and Venus have been closely studied by Pickering in the clear skies of Jamaica. In recent years Pickering has taken a leading part in the observation of the

former planet. He founded in 1913 an international fellowship of astronomers, 'The Associated Observers of Mars', and as head of this organization has had access to many hundreds of drawings made by astronomers all over the world. Since Lowell's death Pickering has been generally regarded as the chief authority on the planet. As to the physical condition of Mars, Pickering has always taken the view that the planet's appearance clearly indicates that its temperature, though lower than ours, is yet sufficiently high to admit of the existence of vegetable life; and this contention was vindicated when in 1924 the astronomers of the Lowell Observatory succeeded in determining the temperature of Mars by radiometric means. As to the canal-system, Pickering's views have never been fixed and static. He has inclined now to one theory and now to another. His most recent view is that the more prominent canals are strips of ground fertilized by 'shower-tracks'—aqueous vapour drawn from the melting polar caps and carried by aerial circulation along curved lines; the finer and straighter canals, he thinks, seem to imply the existence of intelligent life, which 'need not be so very unlike ourselves as we have heretofore been led to surmise'.

Pickering's studies of Venus in 1921 led him to the conclusion that the rotation of the planet is performed in sixty-eight hours with the axis of rotation lying nearly in the plane of the planet's orbit. For well over twenty years Pickering has been keenly interested in the question of a possible trans-Neptunian planet or planets. He and Lowell were together on the track of Pluto, though it was as a result of Lowell's work that that tiny world was discovered. But Pickering believes that there is at least another external planet and perhaps more, and this theme has occupied most of his attention in recent years.

We now turn to the life-work of a third great American observer. **Edward Emerson Barnard** was born at Nashville, Tennessee, on 16 December 1857. His early life was one long

struggle with adversity. The only son of a poor widow with practically no means of support, he was literally born into poverty. His elementary education consisted of two months' schooling, and at eight years of age he was apprenticed to a photographer in his native town. From his early years he was a keen astronomer, and as a mere child was fond of observing the stars with a toy telescope which he purchased out of his scanty savings. By the time he was twenty he had succeeded in procuring a 5-inch refractor, with which he discovered his first comet in 1881. In 1883 he secured a fellowship in the Vanderbilt University and a post in the Observatory, and he graduated in 1887. His fame as a discoverer of comets and a skilled and careful observer had by this time gone out all over America, and in 1888 he was offered and accepted the post of assistant astronomer at the new Lick Observatory in California, which boasted the largest telescope in the world, the 36-inch refractor.

With this magnificent instrument Barnard made, on 9 September 1892, his first outstanding discovery—that of a fifth satellite of the planet Jupiter. His own account of his discovery was as follows:

'Friday being my night with the 36-inch telescope, after observing Mars and measuring the positions of his satellites, I began an examination of the region immediately about the planet Jupiter. At 12 o'clock, as near as may be, to within a few minutes, I detected a tiny point of light closely following the planet, and near the third satellite, which was approaching transit. I immediately suspected it was an unknown satellite.'

On the following evening Barnard again saw the strange object 'rapidly leaving the planet on the following side'. Further observations proved this satellite to be the nearest of all Jupiter's moons to the surface of the planet—112,000 miles away—and also by far the smallest, with a diameter not much exceeding 100 miles. It was evident that this tiny moon was of a different order of celestial bodies from the

four large satellites, and the suggestion was made that possibly this was the first of a zone of asteroidal satellites to be discovered. Four small satellites have since been found, but their orbits are beyond those of the large moons, so Barnard's tiny moon would seem to be an isolated body. If it is one of a group, its companions are too small to be seen or photographed with the most powerful telescopes.

Perhaps Barnard's greatest work was in photographic astronomy. Soon after he went to the Lick Observatory he picked up in San Francisco an old portrait lens at a cheap price. Mounting it equatorially, he began to photograph the sky, and astonished the scientific world by the beauty of the photographs which he secured and the wealth of detail shown thereon. 'We must', said the late Dr. A. A. Common, when presenting Barnard with the Gold Medal of the Royal Astronomical Society in 1897, 'certainly admire not merely the skill but the courage of the man who could, under the very shadow of the 36-inch refractor, demonstrate the merits of a lens which could be bought for a few shillings.'

In 1897 Barnard was transferred to the Yerkes Observatory of the University of Chicago, where the 40-inch refractor had recently been erected. He was appointed also to a Chair of Astronomy in the University of Chicago. At the Yerkes Observatory he made several discoveries of first-class importance, of which the greatest was perhaps that of the tiny swift-moving star in Ophiuchus known as Barnard's star, which turned out to be one of the very nearest neighbours of the Sun. His long-continued study of the Milky Way clouds led him to the discovery of the dark nebulae. As far back as 1905, in describing a nebulous region in Scorpio, he expressed 'a slight suspicion that certain outlying whirls of this nebulosity have become dark and that they are the cause of the obliteration of the small stars near'. These slight suspicions of 1905 became certainties within the next ten years or so. In January 1919 Barnard was able to publish a catalogue of

182 of these dark nebulae, which are presumably the true primeval chaos and of which the bright nebulae are only illuminated portions.

Despite failing health Barnard continued to lead a strenuous life as an observer, and probably overtaxed his declining strength. He died after a brief illness on 4 February 1923, in his sixty-sixth year.

Few names are more honoured among astronomers of all nations than that of 'Wolf of Heidelberg', by common consent the greatest astronomer in the Germany of yesterday. **Maximilian Franz Joseph Cornelius Wolf,** to give him his full name, was born on 21 June 1863 at Heidelberg; and with that ancient university town he was associated all through life. His father, Dr. Franz Wolf, was a well-known medical man in Heidelberg, and it was at Heidelberg's world-famous university that young Max received his education. After taking his degree in 1888 he went to Stockholm to study mathematical astronomy under Gyldén, the Swedish mathematician. His studies there extended over two years.

When he was sixteen years of age Max Wolf became keenly interested in astronomy. His father encouraged him in the study, and had a small observatory erected in his garden. Here Max Wolf observed planets and nebulae and in 1884, while still a student, discovered a comet which now bears his name and is known as one of the family of periodic comets. It revolves round the Sun in six years. On his return to Heidelberg, where he had secured a post as Privatdozent in the University, he went back to observational astronomy and began to experiment in photography. His first photographs of the Galaxy in Cygnus attracted considerable attention, for they revealed the presence of vast masses of hitherto unsuspected nebulosity. Even more interest, however, was aroused by his application of photography to the discovery of asteroids. It occurred to him that an asteroid would, owing to its appreciable motion, be represented on a photographic

plate as a trail instead of a point of light like the stars; for the clockwork motion of the telescope keeps pace with the diurnal motion only, and not with the proper motions of planets and asteroids. On 22 December 1891 Dr. Wolf detected his first asteroid by this method, and in the year following he found fifteen more. Ten years later he applied the stereo-comparator to the discovery of asteroids. This instrument greatly lessened the labour of examining plates for asteroids, for such asteroids appear to stand out from the starry background. For the last forty years Dr. Wolf and his assistants have been responsible for the discovery of hundreds of asteroids.

The first asteroid discovery brought Wolf into public notice. He was only twenty-eight, and simply an amateur observer, but went straight into the front rank of astronomers. The State authorities of Baden, recognizing his marked ability, appointed him to an Extraordinary Professorship of Astronomy in Heidelberg University, and to the directorship of the new Astrophysical Observatory in process of erection on the Königstuhl, Heidelberg. Dr. Wolf presided for the remainder of his life over this renowned Observatory, one of the most famous in all the world. Soon after its foundation, an American lady, Miss Bruce, presented a very fine photographic double telescope of 16 inches aperture, with which most of the stellar and nebular work of the Observatory has been done.

Wolf's photographs of the Milky Way revealed a great wealth of detail: many new nebulae too 'swam into his ken'—large extended nebulosities like the 'North America' nebula, and small and faint nebulae which were found by the hundred. In March 1901 he discovered a 'nebelhaufen' or cluster of 108 faint nebulae in the constellation Coma. Another group was found in Virgo. These are the Coma-Virgo clouds of galaxies of which so much has been heard in recent years and whose distances have been fixed by Shapley at 10 million light-years. An even more significant discovery, made quite

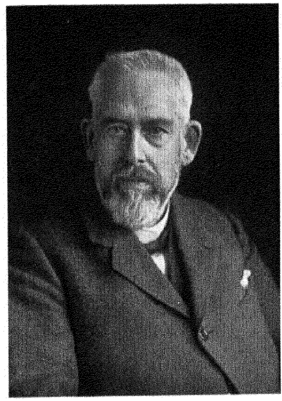
unexpectedly, was that of the dark nebulae. In 1903 Wolf noticed that many nebulae, such as those in Orion and Cygnus, were surrounded by 'regions nearly void of faint stars'. He found that this happened so often that the likelihood of coincidence was very small. At first Wolf assumed that these starless spaces were really 'holes in the heavens'. This was evidently the simplest explanation of the phenomenon. But his study of the peculiar nebula in Cygnus led him to question this interpretation. This nebula, he said,

'is placed centrally in a very fine lacuna, void of faint stars, which surrounds the luminous cloud like a trench. The most striking feature with regard to this object is that the star-void halo encircling the nebula forms the end of a long channel, running eastward from the western nebulous clouds and their lacunae to a length of more than two degrees.... Is there a dark mass following the path of the nebula absorbing the light of the fainter stars?'

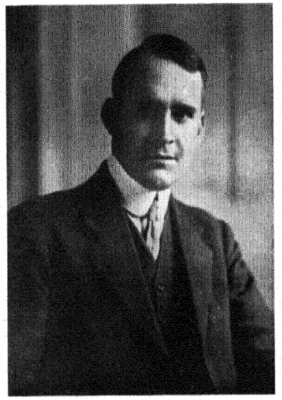
Wolf concluded that this latter supposition was correct, and his investigations were corroborated by those of Barnard. The extended gaseous nebulae were shown to be but small portions of the much larger dark cosmical clouds, which are visible simply because they cut off the light of the faint stars.

Dr. Wolf's individual discoveries are too numerous to enumerate. Besides asteroids and nebulae, they include variable stars, comets, and the temporary star which shone out in Aquila in 1927. The observatory on the Königstuhl may well claim to be one of the world's great centres of discovery and research. It does not boast one of the world's largest telescopes, nor is the climate of southern Germany the finest for astronomical observation. But it had the inestimable privilege of having as its presiding genius for thirty-five years the master-mind of Max Wolf.

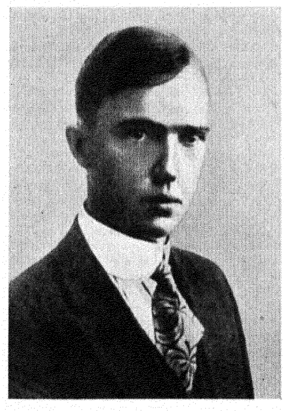
Wolf's activity did not appreciably diminish with the passage of the years. Despite failing health, he retained his professorship and directorship right up to his death, which



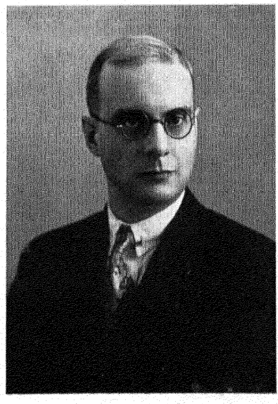
1. Max Wolf



2. Arthur Stanley Eddington



3. Harlow Shapley



4. Pieter Johannes van Rhijn

FOUR MODERN ASTRONOMERS

took place on 3 October 1932, in his seventieth year. The news of his passing occasioned deep sorrow all over the scientific world. One of the kindest, humblest, and most lovable of men, he had many friends as well as admirers, and those who, like the writer, were privileged to enjoy his personal friendship cherish his memory as that of a great and good man.

VIII

EXPLORERS OF THE UNIVERSE

RICHARD ANTHONY PROCTOR—JOHN ELLARD GORE—GIOVANNI CELORIA—HUGO SEELIGER—DAVID GILL—JACOBUS CORNELIUS KAPTEYN—WILLEM DE SITTER—VESTO MELVIN SLIPHER—ARTHUR STANLEY EDDINGTON—HARLOW SHAPLEY—PIETER JOHANNES VAN RHIJN

THE last chapter dealt with a few of the chief 'watchers of the skies' of the last generation—those astronomers who in the main stuck to telescopic rather than to spectroscopic work. In this chapter attention will be given to those astronomers who have concerned themselves above all with the supreme problem of the structure of the Universe, which occupied the attention of the Herschels and Wilhelm Struve.

The first of those astronomers who were first and foremost cosmologists was **Richard Anthony Proctor,** born on 23 March 1837 at Chelsea, London. At the age of thirteen he lost his father, who was a solicitor, and his schooling came to an abrupt end. He entered one of the London banks as a clerk; but his education was only interrupted, his University career postponed. He remained in the bank just long enough to lay aside a little money; then he enrolled at London University, transferring later to Cambridge, where he studied mathematics and theology, and where he graduated in 1860. But he was without means and apparently had no prospect of a career. An early project to study for law was abandoned, and the revival of a boyish interest in nature study led him to take up science. By 1863 he was immersed in astronomy, and he first appeared before the reading public as a writer on the subject in 1865, when his *Saturn and its System* was published. The book brought him into the front rank of writers on scientific subjects, but it was financially a failure; and his pecuniary difficulties were increased by the failure of a New

Zealand bank in which he had invested money. By this time he was married, and for a period he was in acute difficulties. In the course of time, however, he established himself as an authority on astronomy, and succeeded in making a steady, if somewhat precarious, income by writing and lecturing. During this period, said Proctor, 'I did not take one day's holiday from the work which I found essential for my family's maintenance. I would willingly have turned to stone-breaking or any other form of hard and honest but unscientific labour if a modest competence in any such direction had been offered me.'

In the later 'sixties he succeeded in securing publishers for several new books, and in 1868 scored his first financial success with his *Half Hours with the Telescope*. This book was followed by *Other Worlds than Ours*, in which Proctor gave a valuable summary of current knowledge of the planets and adopted and popularized Zollner's view that the giant planets were in a more or less primitive condition. Meanwhile he had procured a telescope, with which he made a close study of Mars, fixing the rotation period to within a fraction of a second. In 1869 he constructed the first really good map of the planet, giving names to 'continents' and 'oceans' and other surface-features and advocating the view that Mars is the most terrestrial of all the planets, 'a miniature of our Earth'.

By this time Proctor was fully immersed in those cosmological researches which gained for him an enduring place in the ranks of modern astronomers. He plotted on a single chart the 324,198 stars of Argelander's *Durchmusterung*, and found quite unexpectedly a connexion between the distribution of the brighter stars and the configuration of the Milky Way—a connexion which on the original disc-theory of Sir William Herschel ought not to exist. 'According to the older theory of William Herschel, we do not come near the boundaries of the Stellar Universe with such a telescope as

Argelander used. . . . There should be no greater number of stars in the Milky Way zone observed with so small a space-penetrating power than elsewhere.' Yet Proctor found that stars brighter than the sixth magnitude were crowded towards the galactic plane. 'In the very regions where the Herschelian gauges showed the minutest stars to be most crowded, my chart of 324,198 stars shows the stars of the higher orders (down only to the eleventh magnitude) to be so crowded that by their mere aggregation within the mass they show the Milky Way with all its streams and clusterings.' Proctor interpreted this result as indicating that the faint stars were really faint, and not merely apparently so because of distance, and that intrinsically bright and faint stars were aggregated together in the galactic zone: in fact that the Milky Way was not so much an optical effect as a region of actual clustering.

It was a much smaller Universe than that envisaged by Herschel that Proctor outlined. The Universe, or Stellar System, he believed to include all the stars visible in the most powerful telescopes as well as all the nebulae, whether gaseous or non-gaseous. The disc-theory of the Stellar System he concluded to be utterly discredited by these researches: further he brought forward evidence to show that Herschel himself abandoned the disc-theory in his later years. Certainly he succeeded in proving from a careful study of Herschel's papers that there had been a development in Herschel's thought and that he had considerably modified his earlier theories and the assumptions on which they were based. But Proctor erred in thinking that Herschel had abandoned the concept of a Stellar System with length much greater than thickness. As a matter of fact Proctor's discovery that the brighter stars crowd on the Galaxy is not now interpreted as indicating intermixture of all sizes of stars in that zone: it is regarded as evidence of the existence of a local cluster whose principal plane is very nearly identical with that of the main Stellar System.

In the course of his charting enterprise Proctor made the important discovery of 'star-drift'. It occurred to him that it would be 'desirable and useful to search for subordinate laws of motion', and that 'if the motions of the stars could be mapped instead of being merely tabulated as hitherto, signs would be traced of such subordinate laws'. Mapping the region of Ursa Major in this way, Proctor found that five of the seven stars of the Plough were drifting 'nearly in the same direction and nearly at the same rate'. Soon afterwards Huggins applied Doppler's principle to these stars, and found their radial motions to be in the same direction and at nearly the same velocity—thus verifying Proctor's important discovery. This was the first recognized case of star-streaming; the existence of subordinate moving clusters is now generally acknowledged, and the Ursa Major cluster has since been found to contain several other stars, including Sirius, the brightest star in the sky.

Proctor was handicapped all through his career by the lack of a dependable source of income. In consequence, he wrote too much, and was prevented from concentrating on those aspects of astronomy which specially attracted him. The wonder is that with so little leisure to give to original research he accomplished so much. Hoping to meet with better fortune in America than in his native England, he settled in Missouri in 1881 and a few years later he transferred his home and private observatory to Florida. While about to sail from New York on a visit to England he was attacked by fever, and died in a New York hospital on 12 September 1888.

Another amateur astronomer whose main work was done in the field of cosmology was **John Ellard Gore.** The son of the Protestant Archdeacon of Elphin, in Ireland, he was born at Athlone on 1 June 1845. Educated at Trinity College, Dublin, he embraced engineering as his life-work and went out to India to serve in the construction of the Sirhind Canal in the Punjab. In the fine climate of India his attention was

drawn to astronomy and he began systematic observations with binoculars and small telescopes.

Retiring from the Government service at the early age of thirty-four, Gore was able to devote the whole of his life to astronomy. For a time he resided with his father; and on the death of the latter he settled in Dublin, where the remainder of his quiet, uneventful, but eminently useful life was spent. In his latter years his sight failed him, and he died as the victim of a street accident on 18 July 1910, at the age of sixty-five.

Double and variable stars claimed much of Gore's attention, and he succeeded in discovering a number of variables; but his main interest in astronomy was cosmological. By quite independent methods he verified Proctor's conclusion that the brighter stars show a tendency to aggregate on the Milky Way. Examining the position of the brighter stars in both hemispheres, he found that 'the number of brighter stars lying on the Milky Way is considerably more than that due to its area'. Stars of each individual magnitude he concluded, taken separately, tend to aggregate on the galactic zone. He interpreted the result as Proctor did, as indicating that stars of all sizes and luminosities were mixed together in the galactic zone; and, like Proctor, he made comparatively modest estimates of the size of the Universe. The Milky Way he believed to be a rifted and irregular ring, 'which marks the equator of a vast globe', with diameter of 20,000 light-years.

Gore's *magnum opus* was his book on *The Visible Universe*, published in 1893, in which he summarized all that was known in the field of cosmology and in which he emphasized the finite nature of the Stellar System. This had indeed been demonstrated in a famous test observation by another distinguished contemporary.

Giovanni Celoria was born on 29 January 1842 at Casale Monferrato, near Alessandria, in Piedmont. After graduating

at Turin University in 1863, and spending some time in Germany, he was appointed assistant to Schiaparelli in the Brera Observatory in Milan. After a long term as chief assistant, he succeeded Schiaparelli as director when the latter retired in November 1900. Celoria held the directorship for seventeen years. He retired in 1917, and died on 18 August 1920, at the age of seventy-eight.

Celoria's chief work was done as a comparatively young man, at the time when his chief was launching out on his career as a planetary observer. His first paper on the distribution of the stars and the structure of the Universe appeared in 1878. With a small refracting telescope of only 4 inches aperture Celoria made a count of the stars in a zone from the equator to 6° north declination extending round the heavens. It was in the course of those gauges that Celoria made his famous test observation in 1879. Near the north galactic pole, his 4-inch refractor revealed exactly the same number of stars as did the large reflector used by Herschel in his gauges. This was a negative result, but a negative result of great importance; for it indicated that in this direction Celoria's telescope had reached the bounds of a Stellar System strictly limited in extent.

Seeliger of Munich may be called the chief German cosmologist after Struve. The major portion of his time was occupied in grappling with this supreme problem. **Hugo Seeliger** was born at Bielitz-Biala, in Silesia, on 23 September 1849. After elementary education in the Gymnasium of the small town of Teschen, he enrolled in 1867 as a student at Heidelberg, where he studied under such giants as Kirchhoff, Bunsen, and Helmholtz. He transferred to Leipzig, where he graduated in 1871 with a doctor's degree awarded for a thesis on the movements of double stars. After a short period of practical work at Leipzig Observatory he was appointed in 1873 to the Bonn Observatory as assistant to Argelander, then nearing the close of his long and useful life. In 1881 he

became director of the Observatory of Gotha, and in the following year he went to Munich as Professor of Astronomy in the University and director of the Observatory in succession to Johann Lamont, a Scotsman who had been naturalized in Germany.

In 1884 Seeliger began his work on the distribution of the stars, which occupied him for many years. His work was based on the *Durchmusterung* of Argelander and Schönfeld, and the star-gauges of the Herschels and Celoria. Seeliger divided the sky into nine zones, each 20 degrees in breadth, by small circles parallel to that of the Milky Way. Thus his first zone or 'region' included the north galactic pole, his fifth 'region' contained the great circle which forms the central line of the Milky Way, while the south galactic pole was included in the ninth. In Seeliger's first region were included 4,277 stars, in the second, third, and fourth respectively 10,185, 19,488, and 24,492 stars. The maximum was reached in the fifth region, which contained 33,267 stars, and it is to be noted that this was the galactic zone. The sixth region contained 23,580 stars, and the seventh, eighth, and ninth respectively 11,790, 6,375, and 1,644 stars. The number of stars gradually increased from each of the galactic poles to the Milky Way itself. It is obvious that if the Galaxy were simply a ring of stars surrounding a star-sphere the number of stars would increase not steadily but suddenly near the boundary of the ring. The conclusion drawn from these researches was that the Universe is flattened at the galactic poles, the number of stars constantly increasing towards the Galaxy itself. In Seeliger's own words: 'The Milky Way is no mere local phenomenon, but is closely connected with the entire constitution of our stellar system.' Following up these researches, Selliger estimated that the distance between our Solar System and the inner border of the 'zone of stellar condensation', that is, the actual Milky Way, is 500 times the distance of Sirius, and the external border 1,100 times that

distance. This placed the limits of the Universe at a distance of about 9,000 light-years from the Solar System.

Seeliger was deeply interested in temporary stars, and in 1892 he advanced the now famous 'star-and-nebula' theory of their origin. According to this theory, the flare-up of a nova is due to the passage of a dark or feebly luminous star through a bright or dark nebula. The theory was accepted with much enthusiasm by many astronomers as perhaps the most likely explanation of temporary stars; but in recent years rival theories have been advanced which find the explanation of novae not in external circumstances but in internal disturbance.

During his forty-two years as Professor at Munich, Seeliger made the Bavarian capital a centre of astronomical thought and activity. He was very highly honoured in his own country, as is evident from the fact that for twenty-four years he was president of the Astronomische Gesellschaft, the most famous of astronomical societies on the Continent. Seeliger continued active up to his death, which took place at Munich on 2 December 1924.

We now come to one whose work in the sphere of astronomical photography entitles him to a secure place among the explorers of the Universe. **David Gill,** the greatest Scottish astronomer of his generation, was born in Aberdeen on 12 June 1843. His father, David Gill, was a watchmaker and jeweller in the city, possessed of ample means; and young Gill, who early developed scientific tastes, entered Aberdeen University. There was a small observatory attached to King's College, and there Gill did his first practical work in astronomy. Shortly afterwards he erected a private observatory in the garden of his father's home, and with his 12-inch reflector he secured some photographs of the Moon. These came to the notice of Lord Lindsay, afterwards Earl of Crawford and Balcarres, who was planning the erection of an observatory on his father's estate at Dunecht, thirteen

miles from Aberdeen. A friendship sprang up between the two men, and in 1872 Lord Lindsay invited Gill to become the first director of Dunecht Observatory. By this time Gill's father had retired, and the old-established and successful business had passed into his son's hands. To accept Lord Lindsay's offer meant a considerable sacrifice. But Gill's love for astronomy impelled him to make the sacrifice, and for four years he directed the little observatory with great ability. In 1874 he led Lord Lindsay's expedition to Mauritius to observe the transit of Venus, in the hope of getting more accurate determinations of the solar parallax, and therefore of the distance of the Sun. This question interested him intensely, and in 1877 he equipped an expedition to the island of Ascension with a view to getting the solar parallax from measures of the parallax of Mars at its nearest. By this time Gill had resigned from Dunecht Observatory in order to follow his own line of working. His measure of the solar parallax proved very accurate, and he was at once acclaimed as one of the leading astronomers of the day. It was no surprise, therefore, when in 1879 he was appointed to the important post of 'Her Majesty's Astronomer' at the Cape of Good Hope.

His early years in South Africa were occupied chiefly with attempts to measure the distances of the brighter stars, some of which he determined with great accuracy. But his most important contribution to astronomy came about almost accidentally. With the aid of a very ordinary camera attached to the equatorial telescope, he secured some very fine photographs of the comet of September 1882. He was impressed by the number of individual stars which appeared on the plate. A brilliant idea occurred to him. For some time past he had contemplated extending the Argelander-Schönfeld *Durchmusterung* from where Schonfeld had left it to the southern pole; and the photographs which he secured suggested to him that the positions of the stars could be

determined by photography with great accuracy, and that much time and unnecessary labour could be saved. The photographic work was commenced in 1885 and completed in 1890. Half a million star-images appeared on the plates. The task remained of cataloguing these. The work of reduction was carried through by J. C. Kapteyn, the Dutch astronomer, and the catalogue was published in 1900.

Gill took a prominent part in the inception of the plan for a photographic chart and catalogue of all stars down to the eleventh magnitude, to be carried through by the co-operation of the Observatories of all nations, and the Cape Observatory was assigned a large part of the work involved in the Astrographic Chart. Other important investigations carried through by Gill were determinations of the solar parallax by means of the asteroids, redetermination of the mass of Jupiter, and studies of the distribution of the stars of different spectral types. The Observatory was largely reorganized and re-equipped in the early years of the century; and in October 1906 Gill retired from the post which he had so long adorned. On his return to Great Britain he threw himself with great vigour into literary work and completed in 1913 his *History and Description of the Cape Observatory*. He died in London after a brief illness on 24 January 1914, and was buried in Aberdeen.

His friend and co-worker, **Jacobus Cornelius Kapteyn,** has been rightly called by de Sitter 'one of the most important personalities in the whole history of astronomy'. Kapteyn was born at Barneveld, in the province of Gelderland, on 19 January 1851. His father was a schoolmaster, who kept a private boarding-school. At the age of eighteen he entered the University of Utrecht, where he studied for six years. Towards the close of his university career, as he expressed it in a letter to the present writer, he 'began to feel a steadily increasing relish for original investigation'. But at this stage his interest was in science as such, and not in any particular

science. His choice of astronomy as a career was in some measure accidental. A vacancy occurred on the staff of the Leyden Observatory, and Kapteyn, then on the look-out for some means of livelihood, applied for the post and was successful. 'Had a place of assistant of physics been vacant, my career would probably have been altogether different.' But, he explained, 'my stay at Leyden was decisive. The refined methods of astronomical observation had a particular fascination for me.'

Kapteyn's stay at Leyden was of brief duration, for at the end of 1877 he was appointed to the newly founded Professorship of Astronomy in the University of Groningen. He had every reason to hope that the foundation of the chair would be followed up by the building of an observatory. 'At first the fulfilment of this hope seemed near enough, but do what I would year after year elapsed without bringing the erection any nearer.' Accordingly, Kapteyn turned his attention to pure mathematics, and for a while returned to Leyden by special permission of his old chief Bakhuyzen, to carry through some investigations in the parallaxes of the stars.

While occupied in this work Kapteyn heard of Gill's project to extend the *Durchmusterung* of Argelander and Schönfeld to the southern sky. He was in correspondence with Gill at the time and learned from him that the task of measuring and reducing the photographs was likely to be too arduous for the staff of the Cape Observatory.

'It has occurred to me', Kapteyn wrote to Gill on 23 December 1885, 'that by measuring and reducing your photographs I could contribute very effectually towards the success of an enormous and eminently useful undertaking. Since then I have revolved the idea in my mind and I have come to the conclusion that if you will let me, and if I can secure the necessary help, there is no one can be in better conditions to undertake the work than myself.'

Gill was only too delighted.

'It will, I hope,' he wrote on 22 January 1886, 'be as satisfactory

to you as it has been to me, that we have mutually and almost simultaneously confided to each other the objects of our work, our hopes and our difficulties,—I with too much on hand, you with too little, both interested in precisely the same kind of work and both intent on having such work done.'

Kapteyn was occupied with this work for fourteen years; for it was not until 1900 that the *Cape Photographic Durchmusterung* was completed.

'It is to my colleague and friend whose name appears on the title page', wrote Sir David Gill, 'that I am under the deepest obligation. At a time of great stress and discouragement, he lifted from my shoulders a load of responsibility by his noble and spontaneous offer to undertake the measurement of the plates, the computation of the results and the formation of the catalogue, . . . I feel assured that Kapteyn has not laboured in vain, and that astronomers will duly appreciate what he has done for their science.'

Soon after Kapteyn embarked on his arduous task of measuring these plates, one of his colleagues in Groningen University lent him two rooms in the physiological laboratory, where the greater part of the work was carried through. Some years after the completion of this great task, another great opportunity arrived and Kapteyn was quick to seize it. In the letter to the present writer from which quotations have already been made, Kapteyn told how

'in 1896 the greater part of the former dwelling of the governor of the province of Groningen was temporarily placed at my disposal. It was impossible to change it into an observatory if for no other reason than . . . that no alteration whatever in the building was allowed. I at once resolved to make it an astronomical laboratory, that is, mainly a place where stellar photographs were to be measured, reduced, and discussed along with investigations to be made of stellar distribution, etc. I think the idea was a happy one.'

In 1903 the former mineralogical laboratory was handed over to Kapteyn, and the astronomical laboratory acquired a permanent home. Such was the genesis of one of the most

important astronomical institutions in the world, the foundation of which was due to the indomitable spirit of a man who refused to be overcome by circumstances and chose to make the most of his limited opportunities.

After the completion of his arduous labours on the *Cape Photographic Durchmusterung*, Kapteyn commenced those cosmological researches which were to occupy him for the next thirty years. In his first paper on the distribution of the stars in 1892 he showed that stars of the first spectral type have smaller proper motions than those of the second. This fact was obviously capable of one of two interpretations. Either stars of the second type move more rapidly than those of the first, or the average velocity of both types is the same, but second-type stars are nearer to the Solar System than first-type. Kapteyn concluded in favour of the latter view. 'The near vicinity of the Sun', he thought, 'contains nearly exclusively stars of the second type'; and he concluded that this assemblage of second-type stars, including the Sun, formed an organic whole which he called the 'solar cluster'. In 1902, however, he abandoned the cluster theory, while still believing stars of the second type to be on the average nearer than those of the first. But eight years later Kapteyn found that second-type stars had on the average greater velocities than those of the first. From the study of radial motions, he concluded in 1910 that the linear velocity of the stars increased from one type to another through the Harvard subdivisions BAFGKM.

In 1904 Kapteyn read at the International Congress at St. Louis, U.S.A., a paper of far-reaching importance, in which he announced the discovery of star-streaming. As is well known, there are two components in the motion of each star: (1) the parallactic motion due to the movement of the Sun, carrying with it the Earth and the other planets, and (2) the individual proper motion. Formerly it was believed that after the parallactic motions were eliminated the

individual motions were more or less at random. This assumption Kapteyn showed to be erroneous. After eliminating the parallactic motion from the motions of the bright stars in the catalogue of Bradley, revised by Auwers, Kapteyn found that the motions of the stars fell into two opposite directions in the galactic plane. This result he interpreted as indicating that the brighter and nearer stars belong to one or other of two vast interpenetrating streams.

This announcement came as a great surprise to astronomers; they were quite unprepared for it. The discovery was, however, decisively confirmed by Eddington and Dyson within the next few years, and the streaming of the stars, or at least the nearby stars, is universally accepted as one of the indisputable facts of astronomy. Various explanations have been advanced, of which perhaps that of Shapley is the most probable, namely that the local cluster consists of two interpenetrating clusters, and that the phenomenon of star-streaming is due to this interpenetration.

In his later years Kapteyn made a frontal attack on the problem of the structure of the Universe. With a vast amount of data concerning proper motions, visual magnitudes, and parallaxes at his disposal, Kapteyn proceeded to sort out the stars, magnitude by magnitude, fixing their approximate distance by means of the relation between parallax and individual magnitudes. Determining the mean parallactic motion—the reflection of the solar motion—of stars of a given magnitude as a whole, Kapteyn was able to get mean distances for stars of each magnitude. The results of his long-continued studies were summarized in several papers published in conjunction with his assistant and successor, Dr. van Rhijn. Assuming the Sun to be near to the centre of the system, or of that part of the system under discussion, Kapteyn and van Rhijn calculated the star-density outwards, and were able to draw a section of the Galactic System. They concluded that they could assume the reliability of these

results up to about 1,500 parsecs. 'In the direction of the pole of the Galaxy this brings us to what many will be inclined to take as practically the limit of the system. . . . In any direction along the plane of the Milky Way, on the contrary, this same limit must be eight times more distant.' This gives 9,000 light-years for the thickness of the Kapteyn Universe and 72,000 for its diameter. Kapteyn, as has been said, worked from within outwards: his 'Universe' is but a small part of the greater Stellar System outlined by Shapley and known as the Shapley Universe.

In virtue of its unique place among scientific institutions and its directors' world-wide reputation, the Groningen Astronomical Laboratory had many links with observatories all over the world. In 1906 Kapteyn launched his famous 'plan of selected areas'—a proposal for the construction for 206 areas distributed uniformly over the sky of catalogues giving magnitudes, parallaxes, proper motions, and radial velocities down to the extreme limits of faintness. This involved the co-operation of various observatories all over the world. In his later years Kapteyn was closely associated with the Mount Wilson Observatory, of which he was a research associate, a connexion which entailed frequent visits to America.

In the Laboratory's early days Kapteyn worked single-handed, but latterly he was provided with one or more assistants. Among his early helpers was Willem de Sitter, now Professor at Leyden. In his latter years he had the invaluable collaboration of P. J. van Rhijn, to whom a great part of the work of the structure of the Universe was due, and who succeeded him on his retirement in 1921. The great astronomer survived this event by little more than a year. He died in Amsterdam, aged seventy-one, on 18 June 1922.

Of the numerous younger men who came directly under the spell of Kapteyn's influence, perhaps the most outstanding is **Willem de Sitter,** now director of the leading observa-

tory of the Netherlands. Born at Sneek in Friesland on 6 May 1872, de Sitter went to school at Arnhem, and from 1891 to 1897 studied at the University of Groningen, where, as stated, he came under the influence of Kapteyn. At this time, however, his chief interest was in pure mathematics, and though he was working in the Astronomical Laboratory under Kapteyn, he had no intention of adopting astronomy as his profession. His ultimate choice was due to Sir David Gill, who in 1896 paid one of his visits to Groningen, met Kapteyn's brilliant student, and invited him to go to the Cape as assistant. De Sitter accepted, sailed for South Africa in 1897, and remained there for two and a half years. On his return to Holland he was appointed assistant to Kapteyn in the Astronomical Laboratory at Groningen. He took up his duties there on 1 January 1900, and for eight years he was the great cosmologist's right-hand man. His own original work in astronomy began to attract attention; and it was no surprise when in 1908 he was appointed to the directorship of the Leyden Observatory and the chair of Astronomy in the historic University there.

It was while working at the Cape Observatory that de Sitter commenced his long-continued study of the system of Jupiter. His own and other observations were made the basis of his classical determination of the masses and densities of Jupiter and its satellites. For the masses of the four old satellites he found the following values, taking our own Moon as unity—0·99, 0·64, 2·13, and 1·17; while the densities are 1.06, 1·00, 0·61, and 0·38 respectively. Dr. Crommelin has remarked on the fact that 'there is a faint resemblance in the arrangement to that of the planetary system, the bodies nearer to the primary having smaller masses and higher densities'.

In the sphere of what is called exact astronomy, de Sitter is an acknowledged master. He has rediscussed all the 'astronomical constants' and suggested revised values for

these in the light of recent investigations. His study of the rotation of the Earth, based on minute irregularities in the motion of the Moon, Venus, and Mercury, convinced him that our planet's rotation is variable within narrow limits. The variations, de Sitter concludes, may 'arise from the abrupt changes in the arrangement of matter in the Earth's interior which take place at irregular intervals'.

In recent years Dr. de Sitter has been mainly occupied with the problems of cosmology. As far back as 1911, when Einstein's restricted principle of relativity had attracted very little attention, de Sitter discussed the effect of this principle on Newtonian dynamics. After the publication of the general theory in 1915, de Sitter showed that, if the theory be true, three consequences must follow: the deflection of the light-rays of distant stars in the gravitational field of the Sun, the shift of spectral lines towards the red in a gravitational field, and the movement of Mercury's perihelion. But de Sitter has done a great deal more than deduce the consequences of Einstein's theory: he has been the co-worker with Einstein in this field, and on the basis of the theory of relativity he has given us the most comprehensive cosmology ever yet formulated.

The word 'universe' has been used in various senses in astronomy. It may be used to denote our Stellar System or part of it; or to signify all the systems within reach of the most powerful telescopes; or to indicate the whole of reality. Thus we speak of the 'Kapteyn Universe' as that part of our Stellar System investigated by Kapteyn; and the 'Shapley Universe' as the whole of our Stellar System with its attendants. But the 'de Sitter Universe' comprises everything that exists. His concept is deduced from the theory of relativity and deductions from that theory as to the amount of matter in the Universe and the distribution of that matter.

The space-time Universe, finite yet unbounded, is, according to de Sitter, 2,000 million light-years in radius, which is

fourteen times as great as the distance of the most distant object photographed by the 100-inch reflector at Mount Wilson. This Universe probably contains 80,000 million galaxies. Dr. A. C. D. Crommelin, in a recent outline of de Sitter's Universe, remarks that 'an electron bears about the same proportion to a pin-head that the pin-head does to the Sun, or the Sun to the galactic system. But the ratio of the diameter of the Galaxy to the diameter of space is much larger, say 1 to 40,000: thus the tightness with which galaxies are packed in space is much greater than that with which protons and electrons are packed in stars or even in the pin-head or than stars are packed in each galaxy.'

Our own Stellar System, according to Seares and van Rhijn, has a population of at least 30,000 million. If we take this as the average population of a cosmic unit, then there are in the de Sitter Universe 60,000 million times 30,000 million blazing suns. Dr. de Sitter certainly leads us into regions where in the poet's phrase 'the spirit of man acheth with this infinity'.

Among those whose observational work has added to our knowledge of the most distant of the celestial systems, the name of **Vesto Melvin Slipher** takes high rank. Slipher was born on 11 November 1875 in Clinton County, in the state of Indiana. He graduated at Indiana University in 1901, and in that year he joined the staff of the Lowell Observatory in Arizona. Percival Lowell had a remarkable capacity for surrounding himself with men of great ability, and at this early stage in Slipher's career Lowell sensed his powers. He latterly became Lowell's chief assistant. The death of Lowell in 1916 was a tremendous blow to the Observatory. It was in a very special way his own creation: he was its owner and founder, as well as its director. But it was indeed fortunate that the chief assistant was a man whose reputation was already world-wide. Slipher's appointment as director was a foregone conclusion, and the Observatory has prospered

exceedingly under his care. He has been ably assisted by his brother, Mr. E. C. Slipher, and by Mr. C. O. Lampland and other distinguished American astronomers.

Slipher's advent to the Lowell Observatory marked a new departure in that institution's work. Lowell was not a spectroscopist: he was a telescopic observer first and foremost. Slipher, on the other hand, had definite leanings towards astrophysics. In 1902 he commenced his classic study of the atmospheres of the four giant planets. He found these spectra to differ considerably more from the solar than had been previously suspected. A hydrogen line in Neptune's spectrum indicated free hydrogen in the planet's atmosphere, and a temperature high enough to resolve water into its constituent elements; and in the spectrum of Uranus a helium line was suspected. In 1908 Slipher made what was up to that time the most elaborate study of the spectrum of Mars that had ever been made. The question which he sought to settle, which had a very definite bearing on his chief's Martian theories, was whether there was evidence of the presence of water-vapour in the Martian atmosphere. Huggins and Vogel had found traces of lines indicative of water; but Campbell had failed to confirm this conclusion, and as the latter was at the Lick Observatory in command of some of the finest instruments in the world, his negative conclusion carried a great deal of weight. But Slipher, in an equally fine climate and with instruments as fine, succeeded in settling the matter when he saw and photographed the lines.

Slipher co-operated with Lowell in the attack on the distant planet Uranus, which was forced to yield up the secret of its rotation period. The Lowell astronomers used Doppler's principle and found the period to be 10 hours 45 minutes in a retrograde direction. In the same year Slipher announced his pioneering results on the radial motion of the spiral nebulae. He found the great Andromeda nebula to be moving at 200 kilometres per second. In 1914 he published

values for fourteen spirals, finding the average velocity to be twenty-five times the average stellar velocities. In 1921 he announced that two spirals, 584 and 936 in the *New General Catalogue*, were receding with the 'unparalleled velocities' of 1,800 and 1,300 kilometres per second respectively.

In 1912 Slipher announced a discovery as unexpected as it was suggestive. Astronomers had been familiar for over half a century with wisps of nebulosity clinging round the stars in the Pleiades; and Dr. Max Wolf's photographs had shown it to be wrapped in nebulosity. But astronomers were scarcely prepared for the startling announcement from the Lowell Observatory. Slipher, photographing the spectrum of the nebula, found that it shines by light 'which is a true copy of that of the neighbouring star Merope and of the other bright stars of the Pleiades', and he drew the obvious conclusion that 'the Pleiades nebula shines by reflected light'.

In 1916 spectrograms were obtained of diffuse nebulosity surrounding the star Rho Ophiuchi. 'It appears', wrote Slipher, 'that the spectrum of this nebula is continuous and so far as can be judged from this weak plate it is like that of Rho Ophiuchi, about which this nebula clusters. . . . The indications are that this nebula is shining by reflected light, as was found to be true of the nebula in the Pleiades.' Slipher noted also the suggestive fact that in both of these regions of the sky faint stars are conspicuously deficient in number. The type of spectrum conforms to the view that the scarcity of stars in these and certain other regions is due to light absorption by nebulae which may otherwise be invisible.

Arthur Stanley Eddington may be easily reckoned the most outstanding English astronomer of the present day. Born at Kendal in Westmorland on 28 December 1882, the son of a Quaker schoolmaster, he received his early education in the Friends' School of his native town. He proceeded to Manchester University, where he graduated as Master of Science, and thence to Trinity College, Cambridge, where,

after a career of exceptional brilliance, he emerged as Senior Wrangler in 1904. Three years later he was Smith's Prizeman. In 1906 he was appointed the assistant at the Royal Observatory, Greenwich, and seven years later Plumian Professor of Astronomy at Cambridge and director of the Cambridge Observatory. Many academic honours have been bestowed on him, and he was knighted in 1930.

Eddington's first important work was on the streaming of the stars. After Kapteyn's announcement in 1904 of the discovery of the two great star-streams, Eddington sought by independent investigation to confirm or disprove it. Kapteyn's results were based on the motions of the brighter stars in the catalogue of Bradley: Eddington examined the motions of a number of fainter stars, down to the ninth magnitude, in the Groombridge catalogue. The result was an unexpectedly decisive confirmation of Kapteyn's conclusions; and Eddington's still more elaborate investigation of 5,322 stars in the General Catalogue of the American astronomer Lewis Boss convinced him that beyond doubt the Groningen astronomer's conclusion was sound.

About 1916 Eddington turned his attention to a problem which at a first glance would seem insoluble by the mind of man—the internal constitution of the stars. Yet by mathematical analysis he has described a star's interior. 'We can now form a kind of picture of the inside of a star,' he says, 'a hurly-burly of atoms, electrons, and ether-waves. Dishevelled atoms tear along at 100 miles a second, their normal array of electrons being torn from them in the scrimmage. The lost electrons are speeding 100 times faster to find a new resting-place.' There is, Eddington showed, a balance between gravitation which tends to induce the contraction of a star, and resisting forces of which the chief is radiation pressure. The ether waves inside the star 'are trying to escape outwards and they exert a pressure on the matter which is caging them in. . . . In all small globes this force is quite trivial: but its

importance increases with the mass of the globe.' There is a critical mass at which the forces are balanced, and in a star with a mass greater than that critical value the radiation pressure would gain the upper hand and the star would suffer disruption. As a matter of fact, the masses of the stars lie within narrow limits: theory is confirmed by observation.'

At first Eddington confined himself to the interiors of giant stars where, it was believed, the perfect gas laws were alone valid. But in 1924 Eddington showed that this assumption was unsound. In previous investigations, ionization—the loss of atoms by electrons at high temperatures—had been neglected, and it had been hastily assumed that a star ceased to behave as a perfect gas when its density was a tenth that of water. 'This', said Eddington, 'is based on a false analogy between the stellar ions and ordinary atoms.' 'The Sun's material, in spite of being denser than water, really is a perfect gas. It sounds incredible, but it must be so.'

It used to be thought that the main source of the heat of the stars was supplied by the energy generated by contraction. Eddington has shown that contraction is quite inadequate to produce the heat required, and that probably the main source is 'the sub-atomic energy which it is known exists abundantly in all matter'. The stars are very much older than they were formerly supposed to be, and the rate of development much slower. The unit of time in a star's life-history is a billion years. And as the stars grow older they lose mass: they literally burn themselves away. It is a fascinating story which Eddington has to tell, and in his popular exposition of his own researches in *Stars and Atoms*, he tells it with all the enthusiasm of one who has explored territories hitherto unknown.

Concurrently with his study of the stars' interiors, Eddington has worked hard on the theory of relativity. He is perhaps the most popular expounder of the theory in the English-speaking world; but he is more than an expounder.

He is one of the few original workers in this field of research. His *Space, Time, and Gravitation*, published in 1920, is certainly a classic. It fell to Eddington to lead the expedition which went to West Africa to observe the total eclipse of the Sun on 28 May 1919, with the object of testing the prediction of Einstein and de Sitter that a ray of light is deflected in a gravitational field. Plates were secured of the eclipsed Sun on a background of bright stars, and it was found that the star-images were displaced in accordance with the relativity theory.

If any modern astronomer is entitled to the designation of the Herschel of the twentieth century, that astronomer is **Harlow Shapley.** Of him also it may be truly said that he has 'broken down the barriers of the skies'.

Born at Nashville, Missouri, on 2 November 1885, he received his academic training in the University of Missouri. After graduation there, he proceeded to Princeton, where he worked at the Observatory under H. N. Russell, and took his degree of Doctor of Philosophy in Princeton University in 1913. In 1914 he was appointed to the Mount Wilson Observatory, where he had the 60-inch reflector at his disposal. At Mount Wilson he spent seven busy and profitable years, during which he emerged as one of the most brilliant and original of the younger astronomers. In 1921 he was appointed to succeed E. C. Pickering as Professor of Astronomy in Harvard College and director of the Harvard Observatory.

His first astronomical work was on variable stars. His study of the orbits of eclipsing binaries led him to the conclusion that stars of the first spectral type were in many cases denser than those of the second, a conclusion at variance with the then accepted view of stellar evolution. The enigmatical short-period variables known as Cepheids also engaged his attention, and in 1914, the year of his appointment to Mount Wilson, he put forward the 'pulsation theory' of these strange stars. Shapley concluded that these are not binary systems,

and that the explanation of their light-changes can much more likely be found in the occurrence of internal or surface pulsations of isolated stellar bodies. 'We may suppose that because of the internal vibration, the photosphere of the star is periodically shattered or broken through by the rush of hotter gases from the interior.' This theory aroused much discussion, and although rival hypotheses are still in the field, it may be said to have met with the widest measure of acceptance.

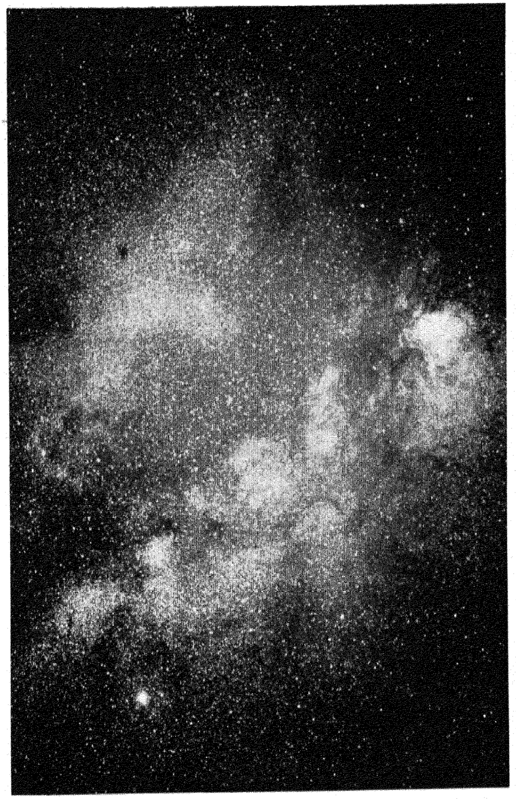
It was in 1914, on his appointment to Mount Wilson, that Shapley entered on his career of cosmological research. He approached the problem of the structure of the Universe by way of a study of star-clusters. The papers in which his investigations were described were entitled *Studies on the Colours and Magnitudes in Stellar Clusters*. These clusters included both the open galactic clusters, which are undoubtedly part of the Stellar System, and the compact globular clusters such as that in Hercules. The Hercules cluster was the object of his first attack. By means of the observed 'colour-indices' Shapley found that the brightest stars in this cluster were red. Bright blue stars, however, were also found, also some Cepheid variables. Assuming that the brightest stars in the cluster were comparable in absolute magnitude to those in the main stellar system, Shapley was able to deduce the distance of the cluster and he corrected this by other methods of computing distances. The distance came out as 100,000 light-years; but a few years later Shapley found this estimate to be nearly three times too great, and fixed the distance at 36,000 light-years.

His work on the Hercules cluster was preliminary to his attack on the globular clusters as a whole. By 1918 he had fixed the distances and positions in space of eighty-six globular clusters—forty-three on one side of the galactic plane and forty-three on the other. The distances ranged from 22,000 to 220,000 light-years; and the clusters were found to

form 'a great roughly-defined ellipsoidal system, symmetrically divided by the plane of the Milky Way'. He defined these as 'cosmic units', dependants of the greater Stellar System, whose principal plane—the Milky Way—was seen to be identical with that of the system of clusters. He found the centre of gravity of the system of globular clusters to be situated amid the dense star-clouds of the Galaxy in Sagittarius, 60,000 light-years from the Sun; and this he at once concluded to be the centre of gravity of the Stellar System, a conclusion confirmed by many other pieces of evidence.

Shapley further showed the Stellar System to be much greater than was believed possible. Faint blue stars in the open cluster M 11 and in neighbouring star-fields were shown to be at least 15,000 light-years away. The Stellar System, said Shapley in 1918, is 'a hundred thousand times as large as we formerly believed it to be'—at least 300,000 light-years in diameter. The majority of the stars, he declared in 1926, 'are found within five thousand light-years of the plane'. 'The Galactic System', he wrote in 1928, 'is an irregularly circular and much flattened system—a conglomerate of single stars, groups of stars, clusters, and great star-clouds, seriously obscured in certain regions by nebulosity.' The whirligig of time brings strange revenges, and here we have the disc-theory again—or something very like it: we have come back to Herschel's later concept of the Stellar System as a greatly extended thin disc, composed both of independent stars and star-clusters.

That our Sun is a star of one of these subordinate clusters was another of the facts brought out by Shapley in 1918. What earlier investigators had believed to be co-extensive with the whole Universe was really a local cluster, and the only central position to which the Sun could lay claim was seen to be a central place in this local cluster; and he suggested that this cluster had originated in the union of two smaller



REGION OF THE STELLAR CENTRE
Photographed by Max Wolf

clusters--traces of this union being observable in Kapteyn's two star-streams.

In 1930 Shapley modified his cosmological scheme. He was led to do so by a study of certain 'clouds of galaxies'. In 1926 he found that a cloud of galaxies in Coma and Virgo, misnamed spiral nebulae, discovered by Max Wolf in 1901, were about 10 million light-years away; and in 1930 he measured the distance of a similar cloud in Centaurus, which came out as 150 million light-years. This Centaurus cloud is the biggest of all known material systems. These clouds of galaxies consist of large numbers of smaller systems, closely connected one with another and yet distinctive. And the sight of these led Shapley to ask if our Galactic System is not comparable to them rather than to the isolated galaxies like the nebula in Andromeda. According to Shapley's revised cosmology, our Galactic System 'is neither an uncommonly great spiral nor a single united star system . . . rather is it a super-galaxy—a flattened system of typical galaxies. Our local system, a star-cloud a few thousand light-years in diameter, appears to be a galaxy similar to the clouds of Magellan or to the typical extra-galactic nebulae.' This interpretation not only fits the facts better than that of 1918, but removes the apparent anomaly in the 1918 scheme—the apparent abnormal size of our system compared to others. But the new theory, Shapley maintains, differs only in detail from the older. 'Dimensions and structure remain much the same: the changes are largely in interpretation.'

Reference has been made to the importance of Kapteyn of Groningen not only as a great investigator, but also as a teacher. More than any other astronomer of recent years, Kapteyn founded what may be called a 'school'. Besides de Sitter, who was first among Kapteyn's distinguished pupils and assistants, several other brilliant young Dutchmen fell under Kapteyn's spell; and thus it has come about that in proportion to population Holland has produced in recent

years more astronomers of first-class ability than any other country. Indeed, their number has been so great that their native land has been unable to offer them employment, and several have gone to America and to South Africa, as well as to the observatories in the Dutch colonies.

One of these younger astronomers has proved himself Kapteyn's direct successor. **Pieter Johannes van Rhijn** was born at Gouda on 24 March 1886. His father was minister of the Reformed Church there, but soon after his son's birth he was appointed Professor of Theology in the University of Groningen, and so it was in that famed university town that the future astronomer spent his childhood and youth. He received his early education at the Grammar School and the Gymnasium, and later passed to the University, where he was the apt pupil of Kapteyn. In 1912 he went to America, and was for two years on the staff of Mount Wilson Observatory. Returning to Groningen in 1914, he became assistant to Kapteyn in the Astronomical Laboratory, and in the following year took his doctor's degree. From 1915 to 1921 he collaborated with Kapteyn in his later work on the structure of the Universe. The work on what is called 'the Kapteyn Universe' was their joint achievement, and the conclusions reached were their joint conclusions. For a time it seemed as if the Kapteyn cosmology and the Shapley cosmology were rival schemes. Kapteyn and van Rhijn criticized Shapley's world-view, and challenged the estimated distances of the Cepheid variables on which it was largely based. Latterly, however, van Rhijn, in the light of further evidence, accepted the Shapley cosmology in its main outlines, and it is now conceded that the 'Kapteyn-van Rhijn Universe' is but a part of the wider 'Shapley Universe'. Kapteyn and van Rhijn attacked the problem by working from within outwards: Shapley by working from without inwards. The two cosmologies are not contradictory: rather they are complementary.

When Kapteyn retired in 1921 from the directorship of

the world-famed institution of which he was the founder and over which he had been the presiding genius, van Rhijn was appointed to succeed him as director of the Laboratory and as Professor in the University of Groningen. And the Laboratory is now something more: it is now, at long last, an Observatory. A fine reflecting telescope has been set up under van Rhijn's supervision on a new dome on the Laboratory roof, and Kapteyn's dream has at last been fulfilled.

In recent years Dr. van Rhijn has busied himself with cosmological problems. Since his return from America, he has maintained a close collaboration with the Mount Wilson astronomers, and he concluded in 1925 an elaborate investigation on the number of the stars in conjunction with Dr. F. H. Seares. Seares and van Rhijn found that 'at the fourth magnitude the ratio of the number of stars per square degree at latitudes 0° and 90° is 3·5. At the twenty-first photographic magnitude the totals per square degree in the Milky Way and at the pole are 73,600 and 1,667 respectively, with a ratio of 44. To the same limit the integrated total for the whole sky is 890,000,000. To the twentieth visual magnitude the corresponding total is 1,000,000,000'; and the possible total in the Stellar System is estimated to be about 30,000 million. 'The separate totals for the latitude intervals 0–20°, 20–40°, and 40–90° emphasize again the importance of the Milky Way as a structural feature of the system. They show that 95 per cent. of the stars are within 20° of the galactic circles, or, stated otherwise, that regions centred on the poles of the Milky Way comprising two-third of the sky include but 5 per cent. of the stars belonging to our system.' Thus van Rhijn indicates the place of the Sun in the scheme of things: one star—and a dwarf star at that—among 30,000 million in the Stellar System; and that system only one of 80,000 million galaxies!

In this brief survey of the life-work of the makers of

astronomy two impressions have been borne in upon us. The history of astronomy is the story of the growing insignificance of the Earth and indeed of the Solar System. If Copernicus was willing in his day to regard our system as the centre of things, Shapley and van Rhijn and de Sitter bid us behold a Solar System shrunk to nothingness—a mere dust-grain amid 'the height, the depth, the gloom, the glory'. But if the progress of astronomy has diminished the importance of man's dwelling-place, it has surely enhanced the dignity of man himself. Themselves creatures of a day, chained to the surface of a dwarf planet moving round a dwarf star which is one of millions in a galaxy likewise one of millions, the makers of astronomy have wrested secret after secret from nature, have weighed the stars in scales and the galaxies in a balance and have sent their sounding-line out to the brink of the Infinite.

INDEX